$7.00 per volume
Single numbers $2.00

QUARTERLY

Volume 5, Number 1
January, 1931

Founded in 1927 by Edward Bradford Titchener and Carl Murchison

THE JOURNAL OF GENERAL PSYCHOLOGY

Experimental, Theoretical, Clinical, and Historical Psychology

JANUARY, 1931

Chaque article est suivi d'un résumé en français
Jedem Artikel wird ein Referat auf deutsch folgen

CEREBRAL CONTROL VERSUS REFLEXOLOGY: A REPLY TO PROFESSOR HUNTER 3
By K. S. Lashley

A STUDY OF THE PHYSIOLOGY OF LEARNING IN ANTS 21
By Hudson Hoagland

THE CONDITIONED REFLEX AND PERCEPTION: THE CONCEPT OF SYNCRETISM 42
By Henri Piéron

THE LABORATORY COURSE IN PSYCHOLOGY: I. METHODS EMPLOYED FOR RANKING STIMULI 53
By Miles A. Tinker

SOME EMPIRICAL TESTS OF THE METHOD OF PAIRED COMPARISONS 64
By J. P. Guilford

SHORT ARTICLES AND NOTES 78

A Note on Peripheral Form Discrimination (Norman L. Munn and George A. Geil) 78

Can Gestalt Theory Save Instinct? (M. Bullard Drury) . 88

Changes in Skin Potential during the Psychogalvanic Reflex (James W. Broxon and Karl F. Muenzinger) . . . 94

APPARATUS 99

A Mechanical Model of the Conditioned Reflex (H. D. Baernstein and Clark L. Hull) 99

A Cathode Ray Oscillograph Apparatus for the Psychological Laboratory (Roland C. Davis) 107

A Measuring Device for the Galvanic Reflex (Roland C. Davis and J. M. Porter, Jr.) 115

An Apparatus for Photographically Recording Piano Playing (Joseph Tiffin) 120

BOOKS 125

Worcester, Massachusetts

Copyright, 1931, by Clark University

Entered as second-class matter December 1, 1927, at the post-office at Worcester, Mass., under the Act of March 3, 1879

CEREBRAL CONTROL VERSUS REFLEXOLOGY

A Reply to Professor Hunter

From the Department of Psychology, University of Chicago

K. S. Lashley

In the October number of the *Journal of General Psychology* Professor Hunter (3) presents a critical review of my theory of the "equipotentiality of cerebral action." The review is far from being a just statement of the evidence upon which the theory was based and shows misconceptions of my point of view which seem fundamental. The reader of the review must gain the impression that I deny the existence of sensory projection areas (p. 457),[1] that I hold that all parts of the cerebral cortex are equipotential for all functions (pp. 457, 465), that I have based the notions of cerebral mass action upon no firmer basis than failure to observe differences in the behavior of operated animals in the maze (pp. 456-57; no reference is made to any other evidence than this), that I have asserted that sensory cues play no part in maze learning, and that I have entirely overlooked the obvious and simple hypothesis which Professor Hunter offers as a substitute for the theory of mass action.[2]

The reader who is familiar with my work should see the inadequacy of the review at once, but those who are not informed of the details of theory and evidence are likely to be misled by the discussion. It is necessary, therefore, to restate the evidence which led me to accept the hypothesis of cerebral mass action as the most probable interpretation of the available facts, especially since the reviewer omits mention of every point which I have stressed and confines his discussion to denial of points which I have never regarded as established or as crucial to the theory.

The questions at issue are two, not necessarily related. The first is

[1]Professor Hunter says, "Lashley's theory . . . does definitely reject the concept of projection areas with the subordinate notions of reflex arc systems and control of behavior by the association of various parts of the cortex." And this in spite of the fact that more than half my work in the last decade has been devoted to study of an admitted projection area, field *w* of the rat's cortex, and much of the rest to the specialized motor area.

[2]It was first proposed by Munk in the early eighties.

the interpretation of the relationship between extent of cerebral lesion and efficiency of performance; the second, my assumption that the maze habit involves something more in the way of central nervous organization than a sequence of direct sensorimotor adjustments, peripheral or kinaesthetic. I shall take up these points separately.

The Evidence for Cerebral Equipotentiality or Mass Action

After obtaining evidence from two independent series of experiments for a fairly close quantitative relationship between the extent of lesion within a given area and the degree of deterioration of a specific function, irrespective of the locus of the injury within the cortical field measurably concerned with the function, I cast about for an explanation. A number of possibilities were reviewed—reliability of the tests, severity of shock or of diaschisis proportional to the extent of destruction of tissue, the existence of an association area more likely to be invaded by large than by small lesions, invasion of subcortical centers, and the elimination of separate sensory or motor functions in a number proportional to the extent of lesion—and were rejected on the basis of what seemed adequate evidence. The only remaining possibility seemed to be that the various parts of the functional area all contribute in the same way to the functions studied and in proportion to their masses. This is in essence the theory of cerebral equipotentiality.[3]

Professor Hunter does not question the observations upon which this conclusion was based, but seeks to show that the evidence against the progressive elimination of sensorimotor aggregates does not justify the abandonment of such an hypothesis for a less definite one. He advances the following arguments:

1. The evidence for mass action is derived "almost solely" from studies of the maze (pp. 465 and 467).
2. The maze is learned in terms of stimuli from many sense fields.

[3]The term *equipotentiality* was used to describe the condition in which lesion to any part of a cerebral area produced no measurable disturbance of function, whereas destruction of the entire area was followed by conspicuous loss of function. More recent data, indicating that cerebral lesion always produces some deterioriation of all the functions of the injured area, have led to the more comprehensive notion of *mass action*. This may be defined briefly as the theory that, within any given functional area of the cerebral cortex, the subordinate parts can individually perform any of the functions of the whole, although at a lower level of efficiency. I have insisted that the theory is applicable only to certain functions which cannot yet be defined in any general terms.

The elimination of any one of these will reduce the animal's capacity to learn, and the elimination of several will reduce the capacity in proportion to the severity of sense privation (pp. 457, 465f.).

3. Destruction of parts of the cerebral cortex will result in the elimination of one to several sensory projection areas, with consequent insensitivity to the sense modes represented. This, like peripheral destruction of the sense organs, will restrict the animal's capacity to learn (p. 457).

4. When a habit involving only one receptor system is studied, there is no evidence for cerebral mass action.[4]

5. The production of greater and greater sensory defect with increasing size of lesion will restrict the animal's capacity to learn as do anaesthesias of peripheral origin, and this restriction by sensory defect is sufficient to account for *all* of the deterioration found after cerebral lesions (p. 465).

6. This explanation is to be preferred to "some intriguing but empty concept of *Gestalt* or other central neural process" (sic) (p. 464).

Studies of Brightness Discrimination. The statement of my critic that the theory of equipotentiality is chiefly based on studies with the maze, and the implications concerning studies of the brightness discrimination habit (4) are incorrect. The first experimental evidence of a relation between cerebral mass and efficiency was obtained in a study of the habit of light-darkness discrimination in the Yerkes box (Lashley, 6), and I still regard this as probably the most significant experiment bearing upon the problem. The facts which I have stressed in discussing this experiment are the following:

1. There is a well defined visual sensory projection area on the occipital cortex of the rat (area *w* of Fortuyn).

2. Lesions to the cortex which do not involve this area or its projection fibers have no influence upon the formation or performance of visual habits, detectable by our present methods.

3. Previous partial or complete destruction of the area does not influence the rate of *initial* formation of the habit of reaction to light versus darkness. I have published data upon 113 cases trained after

[4]Professor Hunter does not explicitly state this, but his arguments on pages 457-459 and 465 imply it. I am puzzled by his treatment of the data on brightness discrimination. Throughout the review he speaks as though this habit were either totally abolished or unaffected by lesions in field *w*, which, of course, is not the case.

more or less complete destruction of this area and on nearly 100 controls. The operated cases are slightly but not significantly superior.

4. Destruction of the whole of field *w after* training results in a total loss of the visual habit, which may be reacquired as by a normal untrained animal.

5. Incomplete destruction of field *w* results in errors in post-operative retention tests, and the amount of training necessary to bring the animal back to errorless discrimination is proportional to the extent of injury within the general region of field *w*. For retention of this habit the different parts of field *w* are equipotential.

There is now additional evidence to show that:

6. After complete destruction of field *w* the ability to distinguish between two lights of unequal intensity is retained but the threshold is usually higher and the rate of formation of habits involving discrimination of slight differences is slowed.

7. Habits based upon the discrimination of slight differences of intensity seem to be abolished by lesions which have little effect upon light-darkness discrimination.

8. Complete destruction of field *w* abolishes permanently the capacity of the animals to distinguish visual patterns, but leaves almost unaffected the capacity to distinguish the position and relative brightness of objects.

The most obvious explanation of the quantitative relation between loss of the habit and cerebral destruction was the production of scotomas proportionate to the size of the lesion, with consequent limitation of the accuracy of reaction in proportion to the restriction of the field of vision. This is in line with the Munk-Hunter theory as applied to a single sensory field.

But I was forced to dismiss this possibility because of the perfectly normal initial learning of animals lacking field *w*, or indeed the occipital half of the cerebrum. For surely, if scotoma interfered with vision in a trained animal, it should also interfere to a measurable extent with the vision of an untrained one. In the experiments, shock, diaschisis, and other general effects of operation seemed well controlled. I considered the possibility that the habit is formed at subcortical levels (Hunter, 3, p. 466), as indeed seems more probable in view of recent work, but this still left the problem unchanged. If the habit mechanism is subcortical, its functioning (when formed in a normal animal) is still conditioned by the quantity of tissue intact

in field *w*, irrespective of the locus of the tissue within the field. It was to explain these facts that I first postulated cerebral mass action. Professor Hunter dismisses them, without discussion, in the statement, "There is nothing, then, in Lashley's results on visual discrimination which is antagonistic to current general theory" (p. 459).

Experimental Control of Sensory Factors in Maze Performance. Fearing some unknown defect in the technique of the Yerkes box which might have led to a spurious correlation, I cast about for another method which might differ enough to render a common source of error unlikely. I finally adopted the maze, reluctantly, with a full realization of its shortcomings, as the only other quantitative technique available.[5] With the maze a quantitative relation between efficiency and extent of lesion was evident, both in initial learning and in postoperative retention (Lashley, 7). The most obvious explanation of this was that which Professor Hunter has revived: that the lesions produce sensory disturbances in proportion to their magnitude. I devised experiments to test this explanation and my conclusions concerning mass action in the maze habit were based more upon the results of these experiments than upon any other consideration (7, Chapter VIII). Since Professor Hunter has not mentioned this phase of the work, which is the most crucial evidence available, I must review it here.

1. A group of normal animals was trained in a maze. They were then blinded by enucleation of the eyes and tested for retention of the maze. They made an average of 6 errors in relearning (p. 111).

Animals with destruction of the visual projection area (field *w*) had made an average of 353 errors in relearning.

2. The somesthetic paths of the spinal cord were transsected in the second cervical segment (Lashley and Ball, 10). (It is not certain that all afferent paths were interrupted, but it is certain that these animals showed more pronounced defects of sensitivity to movement and to posture than has ever been observed after any *cerebral* lesion in any species, including man.) These animals made an average of 16 errors in postoperative retention tests.[6]

Animals with lesions in the somesthetic areas of the cortex (field *j* of Fortuyn) made 145 errors in relearning.

[5]Mr. L. E. Wiley has recently obtained some evidence that the conditions of cerebral function in audition of the rat closely parallel those of vision.

[6]In computing this average, I have included all cases, even those known to have been inadequately motivated during postoperative retention tests.

3. Animals were subjected to cerebellar lesions which produced marked motor and postural disturbances, far in excess of anything resulting from cerebral lesion. Their postural abnormalities must have greatly altered the pattern of kinaesthetic excitation as well. They made an average of 6 errors in postoperative retention tests (Lashley and McCarthy, 11).

The dorsal funiculus, *including the pyramidal tract,* was severed in four animals in the second cervical segment. They made an average of 6 errors in postoperative retention tests.

Animals with lesions involving the motor areas (7, Numbers 75, 80, 84, 87, 89, 97, 100, 102), with some invasion of the somesthetic areas, made an average of 188 errors in postoperative retention tests.

In all these instances, the destruction of the sensory projection area produced a disturbance of function from 9 to 60 times as great as did the destruction of the corresponding peripheral sensory mechanisms. There are three alternative interpretations.

a. The greater defect after cerebral lesion may be due to the involvement of more than one sensory field in every case. We do not know the exact limits of the sensory projection areas and they undoubtedly vary from animal to animal. It is therefore difficult to obtain clean-cut evidence, but the third of the above experiments seems to cover this possibility adequately. Fields *f* and *f'*, which cover the anterior pole of the rat's cerebrum, have been interpreted by histologists as motor; field *j*, which covers the lateral surface adjacent to the orbits, has been interpreted as somesthetic. Physiologically the findings correspond to this interpretation. Fields *f*, *f'*, and the anterior part of *j* are electrostimulable, and extirpation results in largely transient, motor disturbances. Destruction of field *j* produces symptoms indicative of reduced kinaesthetic sensitivity (the animals tend to misplace their feet, fall from a table more frequently than do normals). These symptoms are also transient, and of several cases with complete destruction of field *j* which I have studied recently none could be distinguished readily from normals in posture or control of movement two weeks after operation. There are no other known sensory projection fields within this region of the cortex. The area infraradialis ventralis anterior may also have been destroyed in operations on the frontal pole. It may have olfactory function. Its function is obscure, but it is certainly not a direct olfactory projection area.

We may thus compare the effects of cerebral destruction which

certainly did not involve other than motor and somesthetic projection fields and which produced no evidences of gross motor or sensory loss, observable during the tests, with the effects of spinal lesions which involved the entire pyramidal tract and all of the spinal afferent fibers which are known to have somesthetic function and which produced severe and enduring motor and sensory disturbances. Including cases which involved only the motor and anterior half of the somesthetic fields (thus avoiding any possibility of invasion of the auditory field or of interruption of the more posterior radiations of the thalamus), we find, as stated above, that the cerebral lesions resulted in 31 times as many errors in relearning as did the spinal lesions.

b. The sensory loss from central lesion is greater than that resulting from peripheral sense privation. The cases with cerebral lesions reported under (1) above, with total lesions shown in Figure 27 of the monograph (7) involved only a very slight, if any, injury to the dorsal edge of field *p*, and to the dorsal convexity of the hippocampus. They made 59 times as many errors in relearning as did animals blinded by removal of the eyes. We seem forced to conclude either that destruction of the visual field *w* renders the animal more blind than does removal of the eyes, or that the above interpretation of the data is untenable.

c. The loss of the habit after cerebral lesion is due in large part to the disturbance of some function other than cortical sensory reception—the unknown function which I have called mass action. Some support to the belief in the existence of this additional function of sensroy areas was given by the following experiment (7, pp. 111-112).

4. Animals were blinded by enucleation of the eyes and trained in the maze. The visual cortex (field *w*) was then destroyed and the animals' retention of the maze habit tested. They showed fully as great a loss of the habit as did animals trained with normal vision after the same type of lesion. That is, destruction of the visual area in blind animals was just as effective in abolishing the maze habit as it was in animals which could see the maze during training. Whatever else the source of the loss of the habit in the latter animals, it could not have been the result of cortical blindness.

In addition to these experiments, I have cited other facts which seem to call for some such concept as that of cerebral equipotentiality or mass action.

5. Linear lesions which interrupt a large proportion of the optic radiations have an effect upon the habit of light-darkness discrimination which seems proportional only to the amount of cortex destroyed and not to the area deprived of subcortical connections (6, 7). This is not true for pattern vision, but this fact only complicates the problem without altering the question of the mass relation in brightness vision. The same relatively small effect of linear lesions interrupting large numbers of fibers appeared in the study of the maze, but is as yet less well substantiated. I do not consider this minor effect of linear lesions as an established fact and am continuing the study, but the present evidence for it and its possible fundamental bearing upon neural organization cannot be ignored.

6. Some evidence was presented to show that, in animals operated before training, retentiveness for the maze habit is reduced in proportion to the extent of cerebral lesion, whereas in animals with the visual cortex nearly or completely destroyed before training, there is no evidence of reduced retentiveness for the habit of light-darkness discrimination.

The data upon these points are perhaps not statistically convincing, but they nevertheless call for some consideration. If the rate of deterioration of the maze habit formed after cerebral insult were referable to the sensory functions of the projection areas, then a habit dependent solely upon vision (Hunter 3, pp. 458 and 465) should show as great deterioration in the absence of the visual cortex as does the maze habit in the absence of the projection fields for other hypothetically important senses.

7. Evidence was presented which indicated that with the Miles elevated maze, in the learning of which vision probably plays an active part, the learning of animals with occipital lesions was more greatly retarded than that of animals with lesions in other fields. Since no such difference appeared between the same animals in learning the enclosed mazes, I accepted this as in harmony with the assumption that visual defect after lesion in field *w* is not the primary cause of retardation in learning the enclosed maze.

8. Although all sense modes probably contribute to maze learning in normal animals, there can be little question that some are more important than others. Lindley's data, for example, show a far greater retardation in anosmic than in blind animals. I presented data to show that, when correction was made for the different magnitudes of the lesions in the different cortical fields studied, the amount

of retardation in maze learning was the same for each of the sensory fields, within the limit of accuracy of the experiment (7, pp. 55f.). No differences of the magnitude reported by Lindley were observed.

9. In several reviews (7, 8, 9) I have called attention to the fact that the characteristics of aphasia, visual agnosia, and the like in man are far better fitted by a theory of mass action than by any theory of restricted sensorimotor paths. These symptoms follow lesions outside of the direct sensory projection fields, they show global deterioration of function, and there is some agreement among clinicians that their severity is proportional to the magnitude of the lesion.

These considerations were the ones which led me to the notion of cerebral mass action and the ones which I have most frequently and emphatically stressed. Had my critic questioned the reliability of the evidence, I must have replied that it is for the most part not statistically convincing and that for this reason no single set of experiments can be looked upon as conclusive against his interpretation of the data. But the fact that all lines of evidence which bear upon the problem point to the same conclusion supports the validity of the individual experiments. Wherever I have made positive and general assertions concerning the validity of my views, they have been based, not only upon the experimental evidence at hand, but also upon a wide survey of data upon equivalence of stimuli and of responses, of problems of excitation and integration in nervous tissue, and of problems of cerebral function in man as raised by data on visual agnosia, aphasia, hemiplegia, and dementia subsequent to cerebral insult.

The classical conceptions of cerebral localization and simple sensorimotor organization are not based upon any single decisive experiments, but are valuable and are accepted because they give a plausible and coherent account of a large number of diverse phenomena. I have tried to show that the revision of these conceptions suggested by the notion of mass action is equally plausible and fits an even wider range of facts.

My critic, however, does not question the reliability of the evidence. He confines his attention to a questioning of my observation that there was no significant difference in the maze behavior of animals with lesions in different loci, which could be correlated with differences in sensory capacity. This observation I never urged as final, as is attested by the fact that I sought experimentally controlled evidence of the rôle of sensory defect in loss of the maze habit. And the refutation offered by my critic consists, not in a denial of the

validity of the observations, but in the elaboration of the obvious fact that the rat responds to many sorts of stimuli in learning the maze.

Throughout his discussion, Professor Hunter stresses the rôle of sensory elements in the *learning* of the maze. In the above discussion I have dealt chiefly with efficiency in performance after learning, and intentionally, for it is here that the crucial tests must be made. It is obvious that the animals do make use of sensory cues in learning the maze and that they depend upon the cues in much of their running. But that these sensory cues are the sole determining factor in maze running is less obvious, and data upon the rate of initial learning are not relevant to the question.

Professor Hunter cites the work of Lindley (13) as evidence that sensory defect produces retardation in learning sufficient to account for my results, ignoring the equally valid but less favorable studies of Weaver and Stone (15) and of Liggett (12). Let us see how decisive this evidence from Lindley's study is for the point at issue. Lindley used a different maze from mine but one which was almost certainly more difficult than my maze III, the simplicity of which is emphasized by Professor Hunter. Lindley discontinued training after 30 trials and his blind anosmic group largely failed to finish, but we may compare the excess of errors made by the blind-anosmic group in 30 trials over his normal controls with a similar record of the first 30 trials of my group with lesions in the visual and olfactory centers. The following figures result:

Lindley	Per cent of control	Lashley	Per cent of control
Normal	100		100
Blind	118	Field *w* only	642
Blind-anomic. Based on errors in 30 trials	289	Fields *w* and hippocampus, *ibid.*	540

The animals with occipital lesions were certainly neither totally blind nor totally anosmic, yet their learning was nearly twice as greatly retarded as that of totally blind and anosmic animals.

Thus wherever we have data for a comparison of the effects of sense privation and of destruction of corresponding cortical sensory projection fields, the latter produces by far the more severe disturbance of the maze habit. I have tried to rule out severity of operative shock as a possible basis of the greater deficiency of animals with

cerebral lesions, and Professor Hunter has not questioned the validity of the controls of shock.

It might still be argued that all of my cases with cerebral lesion suffered from defects of other senses than those whose projection fields were directly involved. Such a condition might arise if there were a general cortical depression (shock or diaschisis), or if all senses were to some extent represented in all fields. The first of these I have amply ruled out by showing that vision is not in the least disturbed by extensive lesions bordering on field *w*.[7] The latter involves a view of sensory projection areas more radical than any which I have proposed and is substantiated by no anatomical or physiological evidence.

On the basis of the evidence reviewed above, I must again conclude that the theory of sense privation as an explanation of the relation between extent of lesion and deficiency of performance is inadequate to account for the facts observed. The hypothesis which seems best in accord with the experimental results is that, for certain types of behavior, the projection areas have, in addition to their specific sensory or motor functions, a nonspecific (perhaps facilitative) function in which they are equipotential.

The Relative Vacuity of Concepts of "Central Neural Process" and of "Peripheral Symbolic Process"

Turning now to the second phase of Professor Hunter's criticism, the interpretation of the mechanism of control of the maze habit, we are, I believe, in essential agreement upon every question for which there is direct experimental evidence. Professor Hunter considers that a variety of stimuli play a part in maze learning; the point has always seemed to me so self-evident as not to require special investigation. I have denied that known peripheral stimuli or kinaesthetic-motor chains of reactions are adequate to account for all the facts of maze running; Professor Hunter's results with the temporal maze lead him to the same conclusion with regard to this type of maze. Neither of us has any direct evidence as to the nature of this residual function which may lead to accurate maze running when the possibility of peripheral and known kinaesthetic cues is eliminated. We differ with regard to our speculations. I have stated that "the maze

[7] There is now evidence that even the capacity for pattern vision may be little disturbed by lesions involving as much as half of field *w*, although abolished by complete destruction of one definite small area.

habit cannot be interpreted as a series of kinaesthetic-motor reflexes but must be referred to some intraneural mechanism capable of producing an integrated sequence of movements in the absence of directive sensory cues." Professor Hunter says, "The successful execution of the double-alternation–temporal-maze habit cannot be explained in the ordinary stimulus-response terms where specific cues from the maze or the environment of the maze, or the kinaesthetic processes thereby set up are said to control the behavior. The explanation seems to require the assumption of some symbolic process or some neural engram. The neural engram would be a central control, but it is to be noted that a symbolic process may be peripheral as in the case of human language and as I have elsewhere indicated in commenting upon the delayed reaction. It is an excellent scientific maxim not to appeal to central processes so long as there remains the *possibility* of explaining the given phenomenon in peripheral terms."

Professor Hunter thus rejects the hypothesis of a central-nervous control of the maze habit in favor of a theory of peripheral symbolism, on the ground that the former is "an intriguing but empty concept" which lacks the definite explanatory value of a statement in terms of peripheral processes. I readily admit that the concept of an intraneural mechanism capable of producing an integrated sequence of movements is empty, very empty. But the pot should not call the kettle black! I began life as an ardent advocate of muscle-twitch psychology. I became glib in formulating all problems of psychology in terms of stimulus-response and in explaining all things as conditioned reflexes. I had some small part in the formulation of the theory of implicit speech as a basis of thought which Professor Hunter has used in his theory of verbal symbolism. I embarked enthusiastically upon a program of experiments to prove the adequacy of the motor-chain theory of integration. And the result is as though I had maliciously planned an attack upon the whole system. Accurate movement seemed possible without sensory control (Lashley, 4), studies of tongue- and eye-movements gave no indication of a necessary motor component in thinking, as seems also true of the motor expression of emotions; the conditioned reflex turned out to be not a reflex, not the simple basic key to the learning problem, but to present no less difficult problems than those of the maze habit; I asked what, in terms of neural excitation and activity, are the adequate elements of stimulus and of response (5, 8, 9), and the illusory character of this pseudo-explanation gradually emerged. Stimulus-response is a

psychological category which includes everything or nothing. The assembled students are the stimulus, the professor's lecture the response; the acid on the foot of the spinal frog is the stimulus, withdrawal the response. Clever deduction, perhaps, but does it prove that the mechanisms are in both cases the same, or tell us anything of value about the obvious differences? In order that the concept of stimulus-response should have any scientific value it must convey some notion of how a particular stimulus elicits a particular response and no other, and this requires that we shall analyze stimulus and response in terms of the essential neural excitations involved.

In several places (8, 9) I have summarized a considerable mass of evidence which I believed to show that when viewed in relation to the problems of neurology, the nature of the stimulus and of the response is intrinsically such as to preclude the theory of simple point-to-point connection in reflexes. [The recent discussion by Sherrington (14) shows, in fact, that he no longer considers the older notion of the reflex arc, which has formed the basis of psychological theorizing, as adequate to explain even reflexes.][8] Professor Hunter has not discussed this specific evidence and has dismissed the whole subject with the dogmatic statement, "There is no reason as yet to suppose that the difficulty lies more with current neural theory than with our faulty control and analysis of very complex phenomena."

This brings us back to the maxim, "not to appeal to central processes so long as there remains the *possibility* of explaining the given phenomenon in peripheral terms." Must we abide by this maxim? Is it justified on any scientific or logical grounds? Professor Hunter supports it only by the statement, "We know that behavior can be controlled by receptor processes, whereas we still seek to prove a similar possibility for central processes." On the contrary, any but the most elementary knowledge of the reflexes includes the fact that there is no known reaction which is "controlled" by known receptor processes. All behavior is, indeed, conditioned by sensory processes, but there is in every instance an obscure modifying effect from the remainder of the nervous system.[9] To ascribe this also to receptor processes is merely to beg the whole question.

[8]Eg., "....though trains of impulses are the sole reactions which enter and leave the central nervous system, nervous impulses are not the sole reactions functioning within that system. States of excitement which can sum together, and states of inhibition which can sum together, and states which represent the algebraical summation of these two, are among the central reactions" (14, p. 361).

[9]For an adequate discussion of this point the reader is referred to Goldstein (1).

Is the theory of "sensory control" simpler or more comprehensible than that of "central control"? In the course of time there has been built up a *simple,* traditional, textbook account of the mechanism of reaction, prepared for students' consumption. Repeated copying from one text to another has crystallized it, and early instruction has given us faith in it. The original sources have been almost lost to view and with them the appreciation of the difficulties, the uncertainties, the many unsubstantiated assumptions which underlie every assertion of the classical account. Psychologists are familiar with such crystallizations of doctrine within their own field; they should be wary of the flavor of sanctity in less familiar surroundings.

Professor Hunter's statement of the doctrine "with which Lashley contends that his experimental data are inconsistent," is actually, as far as it goes, an excellent statement of my views. "Behavior is controlled [conditioned] by stimuli arousing neural impulses which pass over reflex arcs and so excite . . . responses. The central nervous system is not regarded as a mere conductor of such impulses, but also as an integrator of them. To this conducting and integrating function current theory also adds the probability of some general neural capacity or plasticity (call it what you will) which sets the limits of accomplishment by the individual." There is nothing in this statement which I have ever called in question nor, so long as the doctrine is expressed in such general terms, is it likely that any experimental data will be inconsistent with it. But I have tried to define the characteristic of the afferent impulses which is important for the control of behavior and have concluded that it is often not their location within specific nerve fibers, as is stated or implied in that part of the traditional doctrine omitted from Professor Hunter's formulation. I have tried to form a more definite idea of the integrative mechanism and plasticity of the central nervous system and have concluded that they involve something more than the shunting of impulses through synapses of different resistances, which is also a basic assumption of the traditional doctrine. I have tried to picture neural integration as an interplay of localized specific activities, modified by some less specific massed effects of excitation (5, pp. 26 ff. 7, p. 171).

I have objected to the kind of psychology which formulates its theories in terms of S and R, where S represents an environmental situation and R a product of muscular activity, and which assumes that S wholly determines R (or many S's many R's), believing that

because its diagrams are simple, neural processes must be equally so. The traditional reflex arc of three neurons is alone inadequate to explain even the simplest known reflex without the postulation of contributory sensory and integrative processes which are as little established as is central nervous control. When the attempt is made to apply the concept of kinaesthetic control to behavior as complex as maze running or language, the consequent speculations concerning persistent patterns of tonic muscular contraction, the equivalence of different motor systems in maintaining the chain of activity, the adequacy of patterns of action currents to serve as neural excitants, and the like, lead us much farther from the firm ground of established fact than does, for example, T. G. Brown's theory of the central maintenance of the respiratory rhythm or Sherrington's statement that, "Central reactions can be much longer lasting than the nerve impulse of nerve trunks."

The theory of peripheral integration is no more simple, no more a matter of common experience, no more supported by direct evidence than is the theory of central maintenance and control of activity. The theories cannot be judged by any a priori rules of methodology, but only by the criteria of correspondence with the known facts and of internal consistency. The facts are, perhaps, inconclusive; consistency Professor Hunter does not seem to have achieved. He refers the control of the temporal-maze habit to "a symbolic process" which "may be peripheral as in the case of human language."[10] This symbolic process is described thus, "Antecedent to the muscular response is a sensory process and, due to the stimulation of receptors by these muscular activities, other sensory processes are consequent to them" (Hunter, 2). The theory of peripheral symbolic processes is thus a modification of Watson's kinaesthetic-motor–chain theory of thinking, and is dependent upon the assumption that the sequence of activities is determined by *kinaesthetic processes initiated by muscular contractions or ultimately by the environmental situation.* But Professor Hunter also says that, "The successful execution of the double-alternation–temporal-maze habit *cannot* be explained in the ordinary

[10]The dogmatism of such a position is illustrated by this citation of language as if it were established as a "peripheral symbolic process." It has been interesting to follow the history of this concept. Beginning as a pure speculation, it has thrived on negative evidence and is now advanced as an unquestioned fact. I know of no evidence which supports the doctrine of implicit speech, and it is opposed at least by the record of several failures to find such evidence.

stimulus response terms where specific cues from the maze or the environment of the maze, or the *kinaesthetic processes thereby set up,* are said to control the behavior" (italics mine).

The notion of central processes controlling sequences of behavior was little more than a denial of the adequacy of the motor-chain theory as an explanation of maze running or of thinking and a suggestion that we can profitably study the central-nervous coordinating processes. As an explanatory hypothesis it is empty, but it may perhaps be filled by continued research. My critic's alternative concept of "peripheral symbolic processes" is likewise empty, and I fear that any content which it may acquire will leak out between its contradictions.

Summary

I am somewhat at a loss to understand the relevancy of Professor Hunter's criticism. I have published and stressed a series of experiments and observations which seemed to support the theory of cerebral equipotentiality or mass action, as applied to certain functions. This material was readily available to my critic. He has ignored it entirely and has proposed, as if new, an alternative theory which I had already dismissed upon the basis of specific experimental evidence. In reply I have restated the evidence upon which I based the rejection of his theory. Since my critic has neither questioned nor discussed this evidence, I can do no more.

References

1. Goldstein, K. Die Topik der Grosshirnrinde in ihrer klinischen Bedeutung. *Dtsch. Zsch. f. Nervenhk.,* 1923, **77**, 7-124.
2. Hunter, W. S. The symbolic process. *Psychol. Rev.,* 1926, **31**, 478-497.
3. ————. A consideration of Lashley's theory of the equipotentiality of cerebral action. *J. Gen. Psychol.,* 1930, **3**, 455-468.
4. Lashley, K. S. The accuracy of movement in the absence of excitation from the moving organ. *Amer. J. Physiol.,* 1917, **43**, 169-194.
5. ————. Studies of cerebral function in learning. V. The retention of motor habits after destruction of the so-called motor areas in primates. *Arch. Neur. & Psychiat.,* 1924, **12**, 249-276.
6. ————. Studies of cerebral function in learning. VII. The relation between cerebral mass, learning and retention. *J. Comp. Neur.,* 1926, **41**, 1-58.
7. ————. Brain mechanisms and intelligence. Chicago: Univ. Chicago Press, 1929. Pp. xiv+186.
8. ————. Nervous mechanisms in learning. In Foundations of Experimental Psychology. Worcester, Mass.: Clark Univ. Press, 1929. Pp. 524-563.

9. ————. Basic neural mechanisms in behavior. *Psychol. Rev.*, 1930, **37**, 1-24.

10. Lashley, K. S., & Ball, J. Spinal conduction and kinaesthetic sensitivity in the maze habit. *J. Comp. Psychol.*, 1929, **9**, 70-106.

11. Lashley, K. S., & McCarthy, D. A. The survival of the maze habit after cerebellar injuries. *J. Comp. Psychol.*, 1926, **6**, 423-433.

12. Liggett, J. R. An experimental study of the olfactory sensitivity of the white rat. *Genet. Psychol. Monog.*, 1928, **3**, 1-64.

13. Lindley, S. B. The maze-learning ability of blind and anosmic rats. *J. Genet. Psychol.*, 1930, **37**, 245-267.

14. Sherrington, C. S. Some functional problems attaching to convergence. *Proc. Roy. Soc.*, 1929, B, **105**, 332-362.

15. Weaver, H. E., & Stone, C. P. The relative ability of blind and normal rats in maze learning. *J. Genet. Psychol.*, 1928, **35**, 157-177.

University of Chicago
Chicago, Illinois

LE CONTRÔLE CÉRÉBRAL ET LA RÉFLEXOLOGIE

Réponse au Professor Hunter

(Résumé)

Le professeur Hunter a douté de la valeur de la théorie de l'équipotentialité cérébrale ou action collective parce que la perte sensorielle progressive avec l'accroissement de la grandeur de la lésion suffit pour expliquer le rapport entre la grandeur de la lésion et le degré de détérioration lequel j'ai trouvé dans l'habitude du labyrinthe et celle de discrimination de clarté. J'avais déjà considéré cette explication en termes de défaut sensoriel et je l'avais rejetée sur la base de l'évidence expérimentale spécifique qui montre que la retardation de la lésion cérébrale est toujours beaucoup plus sévère que celle de la privation sensorielle périphérale correspondante, que le rapport de masse a de la valeur pour une habitude sensorielle quand les lésions sont limitées au seul champ sensoriel de projection, et que la perte des habitudes a lieu après lésion à l'aire cérébrale visuelle chez les animaux aveugles pendant l'acquisition des habitudes. J'ai rapporté de nouveau cette évidence en assez de détail. Mon critique n'a mis en doute ni n'a discuté cette évidence à aucun égard et il faut que je conclue encore pour cette raison que sa théorie n'est pas en accord avec les faits.

Lashley

DIE VORHERRSCHAFT DES GEHIRNS UND REFLEXOLOGIE

Eine Antwort Auf Professor Hunter

(Referat)

Professor Hunter hat an der Rechtsgültigkeit der Theorie der zerebralen Equipotentialität oder Massenwirkung ('mass action') darum gezweifelt, weil der fortschreitende Verlust an Sinneswahrnehmungen der mit fort-

schreitender Grösse der Verletzung einhergeht s.E. genügt, die Beziehung zwischen der Grösse des Schadens und dem Grad der Verschlechterung, welche ich an Gewohnheiten bei der Lösung von Labyrinthproblemen ('mazes') und bei der Unterscheidung von Helligkeitsunterschieden beobachtet habe, zu erklären. Ich habe diese Erklärung mittels Sinnesmangels schon berücksichtigt, und sie darum zurückgewiesen weil spezifische Versuchsergebnisse zeigten: 1) dass die Hemmung auf Grund zerebralen Schadens immer viel ausgeprägter ist, als die durch entsprechenden Sinnesmangel verursachte; 2) dass das Massenverhältniss sich auch dann bewährt, wenn sich die Schädigungen auf ein einzelnes Sinnesprojektionsfeld ('sensory projection field') begrenzen, und 3) dass Verlust der Gewohnheiten nach Schädigung des zerebralen Gesichtsfeldes ('visual area') bei Tieren stattfindet, die während der aneignung der Gewohnheiten blind waren. Ich habe diesen Beweis etwas ausführlich wiederholt. Mein Rezesent hat von diesem Beweis nichts zur Rede gestellt oder besprochen, und ich muss also nochmals folgern, dass seine Theorie den Tatsachen nicht entspricht.

LASHLEY

A STUDY OF THE PHYSIOLOGY OF LEARNING IN ANTS*

From the Laboratory of General Physiology, Harvard University

HUDSON HOAGLAND

I

The attempt to consider learning as a single process has led to much confusion. Adaptation and various forms of physico-chemical facilitations, both inside and outside of the organism, make difficult the technical use of the concept "learning." Learning is generally regarded as a modification of behavior with respect to the environment, due to properties of a central nervous organ; in reviewing neurological interpretations of learning Lashley (13) describes a variety of experiments which indicate the participation of central nervous mechanisms. Central nervous mechanisms which produce conditioning of responses are, however, profoundly obscure, in spite of very marked advances in knowledge of the nature of the nerve impulse and considerable speculation concerning synaptic processes.

Aspects of learning have been investigated both by psychologists and by physiologists. Pavlov (14) and others using modifications of his conditioned reflex technique have contributed markedly to the field. Psychologists have measured the rate of acquisition of maze habits (Hunter, 12), the learning and retention of nonsense syllables (Ebbinghaus, 8), and the development of proficiency in numerous forms of exercises such as typewriting and telegraphy. Most of the psychological work on learning has contributed comparatively little to an understanding of central nervous mechanisms, and the work of the conditioned-reflex school has primarily concerned itself with analysis in terms of physiological reflex units rather than in terms of physico-chemical dynamics.

From the work on learning, despite obvious difficulties inherent in diverse forms of measurement, there has been obtained a certain generalized learning curve. This is characteristically "*S*"-shaped or sigmoid when one plots the amount learned as ordinate against the

*Accepted for publication by W. J. Crozier of the Editorial Board and received in the Editorial Office, August 19, 1930.

number of experimental repetitions of the learning act as abscissa. The sigmoid relation appears, provided that the organism begins the experiments in an unconditioned state, with respect to the act to be learned, and provided that the task is not one too readily mastered.

The sigmoid shape of the learning curve and the approximately hyperbolic nature of the curve of forgetting led Robertson (15, 16) to suggest that learning might depend on the accumulation, by a first order autocatalyzed chemical reaction, of a facilitating substance somewhere in the reflex arc, and that forgetting might depend upon the washing away of this (colloidal) substance. His hypothesis of a molecular basis for conditioning is superficially somewhat similar to Sherrington's (18) proposal of excitatory and inhibitory substances to account for the phenomena of spinal reflexes. In the absence of direct experimental evidence, however, the mere shapes of the learning and forgetting curves can have little specific diagnostic significance, owing to a variety of possible theoretical alternatives. In a discussion of the theoretical basis for certain sigmoid curves Hoagland (11) pointed out some of these alternatives and tentatively suggested, in the case of learning, that the sigmoid nature of the relation might be due to the fact that the curve is essentially an integral distribution curve for thresholds of activations of central nervous units, repetition of the conditioning bringing into play more and more of these units. Such an hypothesis is consistent with Lashley's interpretations of his experiments that the degree of retention of maze habits in rats is directly proportional to the amount of cortex or number of neurons left intact after extirpation rather than to the specific localization of the remaining tissue.

II

Crozier and his collaborators (1, 2, 4, 5, 10) have used temperature as a means of analysis of aspects of physico-chemical relations underlying a variety of vital processes. It seemed possible to extend this procedure to the mechanism of conditioning in cold-blooded animals. In general, for example, aside from more specific possibilities, the temperature analysis with suitable organisms might furnish an experimental approach to Robertson's hypothesis. By keeping the organism at a given temperature and later measuring its retention of learning, facts concerning the dynamics of the "forgetting" process might be indicated.

Ants were chosen because of their availability, hardiness, genetic homogeneity, and educability. Two colonies of the common carpen-

ter ant, *Componotus herculeanus pennsylvanicus* de Geer, were studied. The experiments with the first colony were primarily concerned with standardization of technique and show less quantitative consistency than do the results with the second colony. Unless specifically noted to the contrary, the data of the following experiments were obtained with ants of the second colony.

Ants were taught, one at a time, to escape from a very simple maze placed in an accurate thermostat previously described by Crozier and Stier (7). The ant was required to run this maze without errors for a certain arbitrarily fixed number of times, thus establishing an end-point for "learning" which was used for purposes of comparison at different temperatures. The times for solution were recorded with a stop-watch. Both the time taken to achieve solution of the maze, and also the number of trials required were used as measures of "learning." After the conditioning had been measured in this way, the ant was placed in the thermostat at the temperature at which the experiment had taken place and left for 18 hours. At the end of this time it was returned to the maze in the thermostat, and its retention of learning was measured by observing the number of trials it then took to reach the same end-point of learning that it had reached on the previous day. By using many ants (130 of Colony 2 and 62 of Colony 1) it was possible to measure learning and retention as a function of temperature. There seemed to be no significant differences in the performances of ants of different castes.

The maze is diagrammed in Figure 1. It consists of a brass cylinder 2.8 centimeters in internal diameter with a fine-mesh screen floor and a removable glass top. Below the cylindrical upper part, and continuous with it, is an inverted conical chamber. A vertical brass partition divides the entire apparatus into halves. A brass tube enters each of these halves at the apical end of the inverted cone. There is a small arch-shaped hole in the middle of the dividing partition at the floor level, so that the ant can pass from one side of the divided chamber to the other. All the metal parts were gold-plated. A continuous stream of moist air was pumped through the immersed maze through 30 feet of coiled copper tubing in the thermostat bath, and out through a brass tube fixed in the glass top; the tube straddled the partition.

The ant was placed in the maze, the lid was put on, and the apparatus was immersed in the bath for at least 10 minutes prior to the experiment. During part of this time the ant usually ran about

for some minutes, often passing from one side of the cell to the other through the hole in the partition. After a few minutes the ant usually became quiescent and remained resting on one side of the maze. The stimulus was supplied by deflecting the air, by suitably arranged valves, through a wash-bottle containing peppermint oil in series with

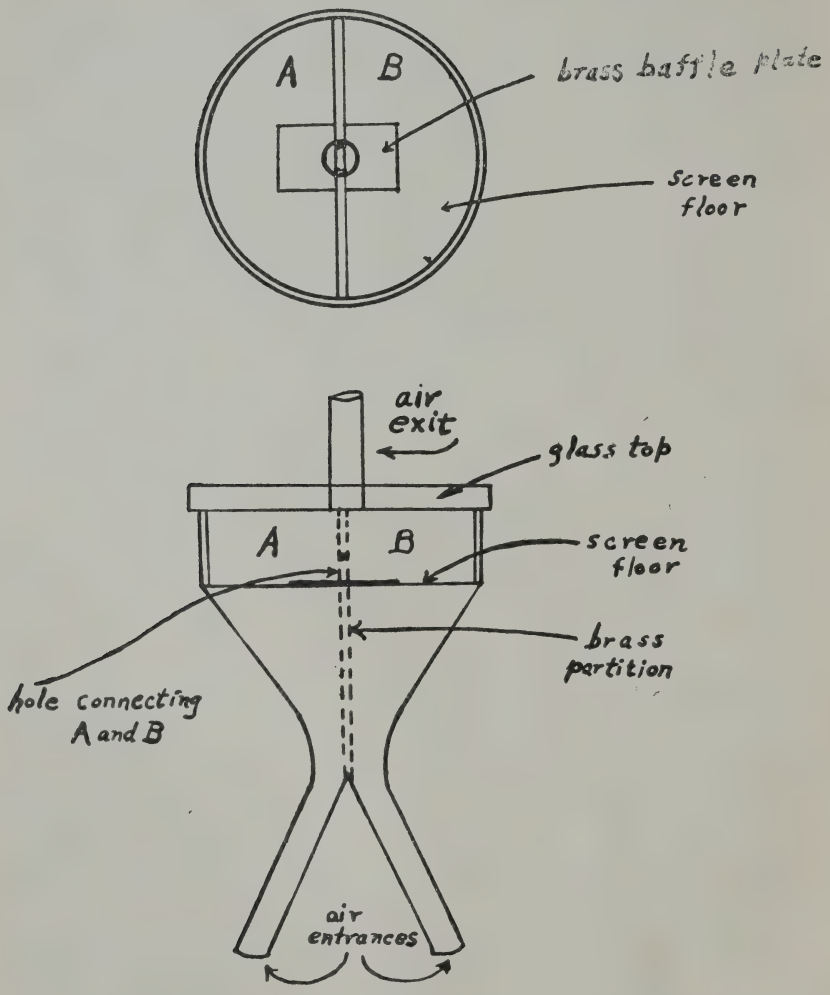

FIGURE 1
DIAGRAM OF THE CHAMBERED "MAZE" DESCRIBED IN THE TEXT

a second 30-foot coil of tubing in the bath and passing the peppermint-charged air through the chamber in which the ant was resting. This procedure invariably induced violent running about on the part of the quiescent ant. For control purposes, in some experiments, an electrically driven buzzer which tapped rhythmically on the metal support of the maze was used instead of the peppermint stimulus. This also caused the ant to run about vigorously.

By passing through the hole in the partition, the only mode of escape from the perfumed half of the cell, the ant entered a chamber relatively free of peppermint oil. At the same time the experimenter, by means of valves, stopped the peppermint charged air and sent pure air through both sides of the maze. At first the ant took perhaps a minute to blunder through the hole, but after a certain number of trials (see Table 1, and Figure 3) the time was reduced to a few seconds, the ant passing directly through the hole after a few quick orienting movements and without making errors or blunders. After escape a rest of exactly one minute was allowed. The current of oil-charged air was then sent through chamber *B* and the ant was driven back to the side from whence it had originally come. In this way the ant could be driven back and forth across the partition, the duration of exposure to the stimulus depending on the time required by the ant to learn to go through the hole. The reaction was specific, the early stages being characterized by blundering and the "learned" stage manifesting itself by an orderly and oriented rush through the hole at the beginning of stimulation. The maze was never cleaned and hence soon became saturated with ant odors. This fact, combined with that of the type of stimulus used and the general behavior of the ant in the maze, precludes the possibility that the "learning" may have been due to the following of odor trails. In order to learn the maze most of the ants were forced to overcome a tendency to negative geotropism which prevented easy discovery of the hole in the partition at the floor level. Mere following of the wall stereotropically and blundering through the hole was not considered a conditioned response; in the maze described, few reactions were of this type. A "correct" solution was usually unmistakable; the ant, after a few "waltzing" movements when the stimulus was first presented, would orient and pass through the hole.

An arbitrary, predetermined end-point of learning in terms of amount of practice was necessary in order to compare the results at different temperatures. The position of the end-point should not

affect the shape of the curve of learning as a function of temperature, since it is the relative effect of temperature in which we are interested. For comparative purposes several arbitrary methods of determining an end-point of "learning" were used. One of these end-points was fixed as the number of trials at the termination of an uninterrupted sequence of five perfect solutions. A more satisfactory end-point, and the one most used, because it required a greater degree of practice on the part of the ant, consisted in scoring one point for each error-free solution and penalizing the ant by halving its score for each run in which a blunder was involved. A score of 10 such units constituted the end-point according to this second method. This second method, involving penalties, merely shifts the end-point further along on the curve of learning. The two measures are directly proportional.

TABLE 1
(Peppermint oil stimulus)

°C.	No. of trials, with probable errors, to score 10 with penalty system	No. of ants	No. of trials before 5 "correct" solutions in a row	No. of ants
15.0	25.3±1.1	15	17.3±1.2	13
19.4	25.2±0.9	10	18.3±1.2	9
25.0	25.6±1.2	19	16.8±0.9	23
27.1	17.4±0.5	10	12.2±0.9	10
28.3	14.6±0.5	10	9.1±0.7	10
29.4	12.8±0.8	10	8.6±0.5	21

Table 1 shows the relation of temperature to the rate of "learning," where learning is defined in terms of the two indicated end-points.

These data show that "learning," assumed to be inversely proportional to the number of trials until the end-point is attained, is relatively unaffected by temperature between 15° and 25° C., but is accelerated about 100% for the range 25° to 29.4°. These relative effects are independent of absolute differences resulting when one or the other of the two end-points is used. It is this relative effect of temperature in which we are interested. Temperatures below 15° were not used, but Mr. N. B. Friedman, working under my direction, trained ants of the same colony to make a right-angle turn in a simple glass T-tube maze. He found no significant differences in

learning rates at 10° and at 20°. The lack of the temperature effect on learning up to 25° is especially striking inasmuch as the rate of creeping was observed by both Mr. Friedman and myself to increase rapidly and progressively over this range. Data of Shapley (17) on the rate of creeping of the ant *Liometopum apiculatum* show linear relations when plotted by Crozier (1, 2) according to the Arrhenius equation. Between 16° and 38.5° a value of $\mu = 12{,}220$ was obtained, while below 16°, $\mu = 25{,}000$. The dispersion of the data is remarkably small. Temperature thus markedly and progressively accelerates the rate of creeping in this ant over a range of from 10° to 38.5° C.

Figure 2 is a direct plot of "learning" as a function of temperature, and Figure 3 is a plot of "learning" as a function of the temperature according to the Arrhenius equation

$$\frac{k_2}{k_1} = e^{\frac{\mu}{R}\left(\frac{1}{T_1} - \frac{1}{T_2}\right)},$$

where k_1 and k_2 are constants assumed to be proportional to the underlying velocity constants of the process at temperatures T_1 and T_2, Kelvin scale, R is the gas constant, e the base of natural logarithms, and μ has the dimensions of calories per gram molecule of reactant for a unimolecular change. Approximately linear relations are obtained between 25° and 29.4° in both Figures 2 and 3. A wider temperature interval would be necessary in order to distinguish between the two functions represented in the plots.

The lack of effect of temperature on conditioning up to 25° is consistent with the notion of a physical process controlling the reaction. On the other hand, since learning may be complex, opposing effects, the rates of which may be influenced oppositely by temperature, may annul one another below 25°. For the range of 25° to 29.4° the value of μ is 7300 calories which agrees with certain other physiological processes found for this temperature interval (cf. 6). The very short range of temperature makes the calculation of μ difficult, and the value of 7300 calories is to be regarded as only roughly indicative. The occurrence of a critical temperature at 25°C. is also significant. This temperature has been found to be a critical one for a variety of chemical reactions controlling vital processes (cf. 3). It is clear from Table 1 that the shape of the temperature

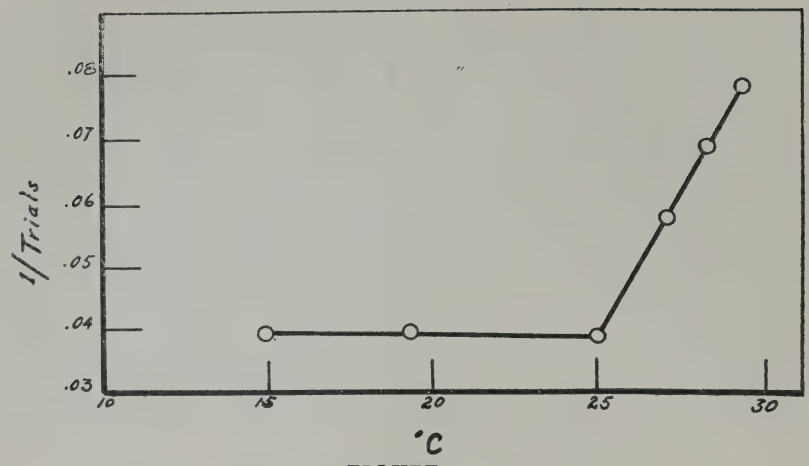

FIGURE 2

DIRECT PLOT OF "LEARNING," MEASURED BY THE RECIPROCAL OF THE NUMBER OF TRIALS TO REACH AN END-POINT, AS A FUNCTION OF TEMPERATURE

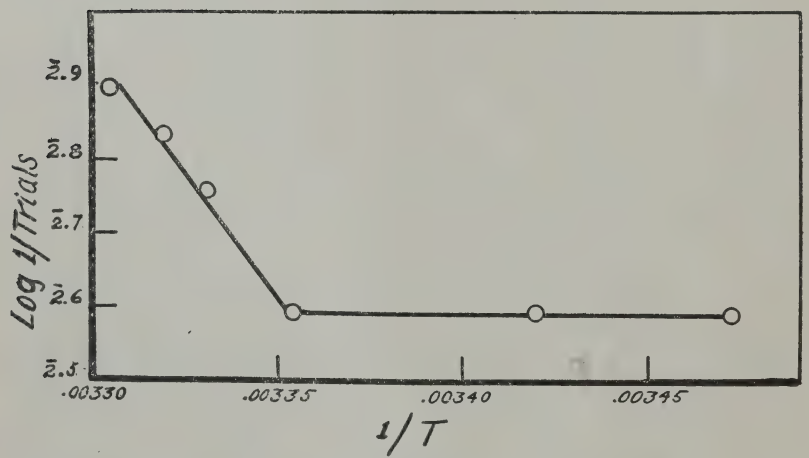

FIGURE 3

PLOT OF THE LOGARITHM OF "RATE OF LEARNING" AS A FUNCTION OF TEMPERATURE ACCORDING TO THE ARRHENIUS EQUATION

The value of μ for the range of 25° to 29.4°C. is 7300 calories.

vs. learning curve, the position of the critical temperature, and hence the value of μ are independent of the end-point used to measure "learning." It is hoped in the future to extend the temperature analysis to other forms of conditioning of ants and also of other poikilothermous animals and to compare the results.

In order to show the effect of temperature on the time factor in learning, the total number of trials for each individual in each experiment was divided into five equal parts and the times for each of these fifth parts were averaged together for all of the ants of Colony 2 at each temperature. By plotting each of these average times for each fifth part of the experiments, as ordinate, against the fifths, as abscissa, expressed as percentage of trials to completion, it was hoped to show the effect of temperature on the time of solution when the lengths of all of the experiments as measured by the number of trials were taken for comparative purposes as 100%. Figure 4 is the result. The rates of solution are seen to increase progressively in all cases as the experiments proceed but are most affected above 25°. The curves are essentially similar for 15°, 19.4°, and 25°.

Since the time of learning as used in Figure 4 also includes a factor involving the effect of temperature on speed of creeping, it was necessary to subtract from the measured times the duration of the first fifth of the tests, as these durations were found to decrease progressively with rise of temperature, although no appreciable learning had occurred. This reduction to a zero origin is illustrated in Figure 5 and indicates the effect of temperature on time for learning independently of time for running.[1]

The time factor clearly serves as an independent check on the analysis in terms of the number of trials. For the range of 15° to 25°, temperature seems to have little effect on the increased rate of learning while for the range of 25° to 29.4° rapid and progressive acceleration is apparent.

If learning depends upon the elimination of random movements through the functional activity of a central nervous mechanism, one might expect that the reciprocals of the probable errors of the

[1]Of course some learning does occur in the first 20% of the trials, but this cannot be corrected for. Because of this error, and also because the time factor is inevitably complicated by the problem of adequacy of stimulus, "drives," etc., it has not been regarded as sufficiently reliable to use in calculating μ.

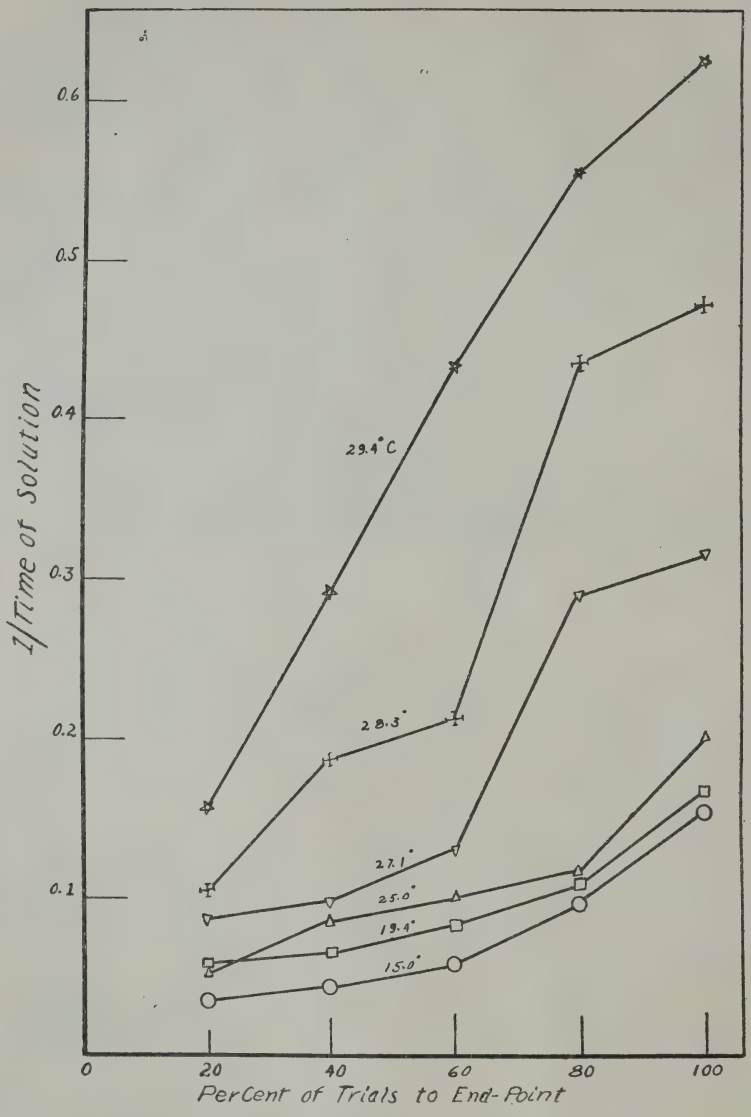

FIGURE 4

PLOT OF TOTAL SPEED OF SOLUTION AS A FUNCTION OF THE PERCENTAGE LEARNED FOR SIX DIFFERENT TEMPERATURES

times for solution, using a constant number of foci of variation (10 ants per point), would be a directly proportional measure of the progress of "learning" (cf. 4). Strictly speaking, for this type of analysis, there should be equality at each point of both the number of

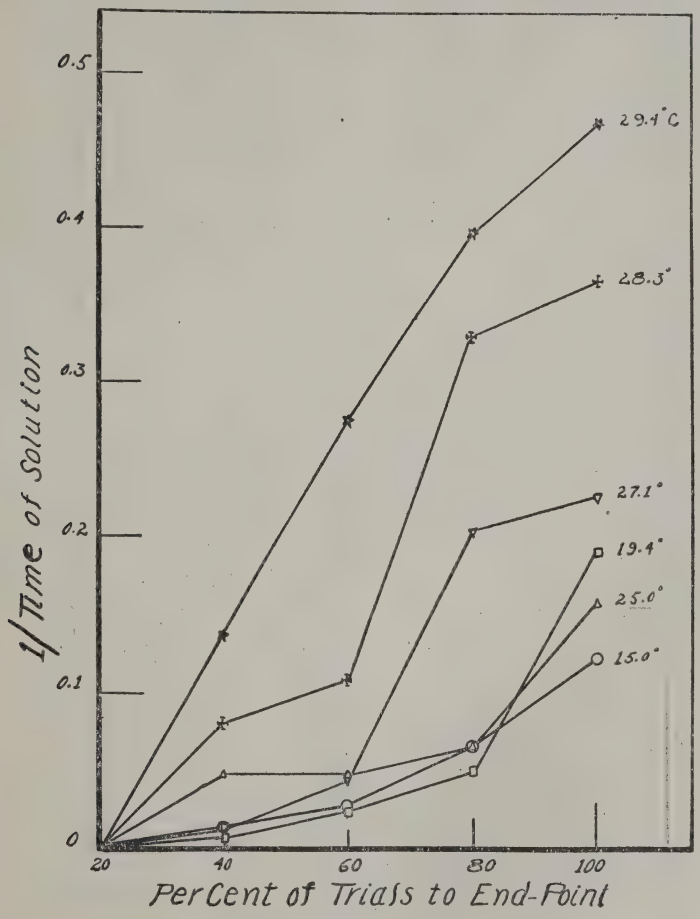

FIGURE 5

PLOT SHOWING SPEED OF "LEARNING" AS A FUNCTION OF THE PERCENTAGE OF LEARNING ACCOMPLISHED FOR SIX DIFFERENT TEMPERATURES

The reduction to a zero origin, by subtracting the speeds for the first 20% of the trials in which learning is assumed to be negligible, eliminates, in this plot, the effect of temperature on the speed of running.

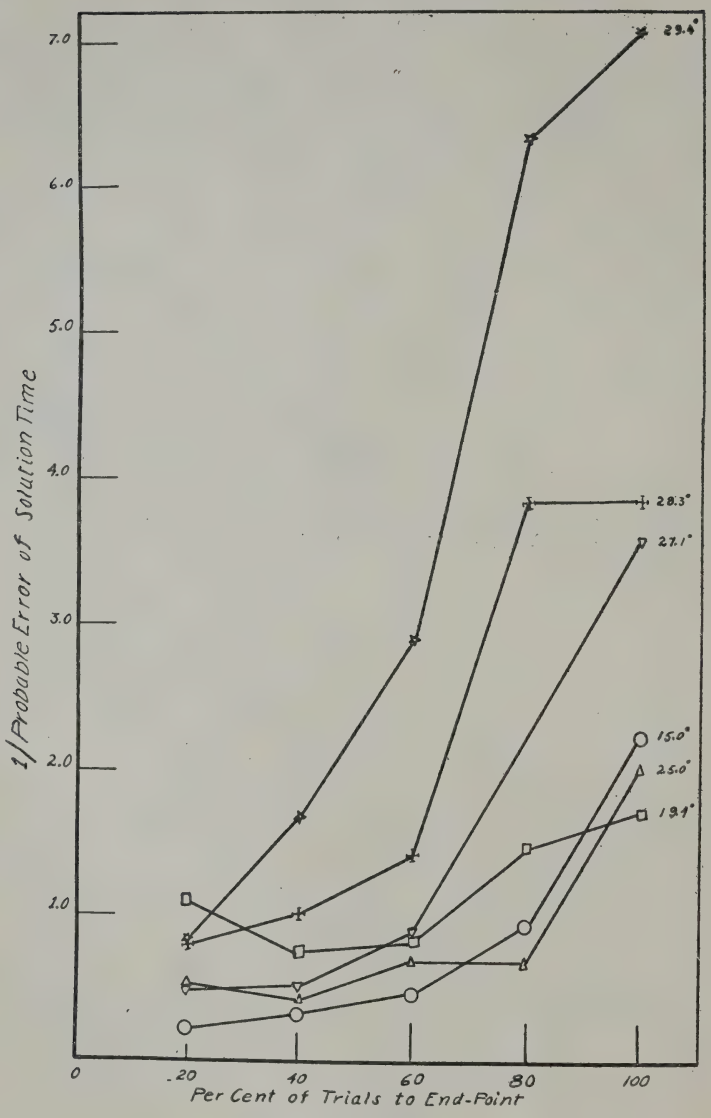

FIGURE 6

Plot of the reciprocals of the probable errors of the times for solution, assumed to be a direct measure of the physiological determinants of learning, against the percentage of completion of learning.

individuals used and of the number of observations per individual. Owing to the method of averaging the times for the fifths of the experiments there are actually somewhat more observations per point per animal below 25° than above it, due to the greater total number of trials required to learn at the lower temperature. Figure

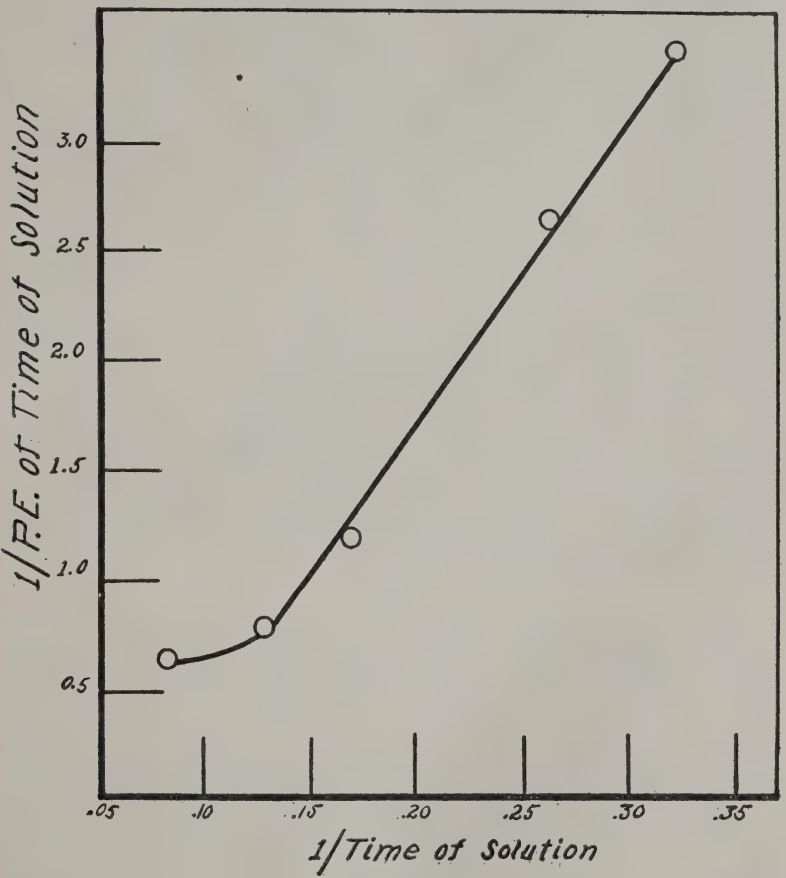

FIGURE 7

Plot of the reciprocals of the times of solution against the reciprocals of the probable errors of the times for mean 20% intervals during the course of the experiments. Both of these coordinates are assumed to be direct measures of the underlying physiological determinants of "learning." The linear relation after the first 20% of the trials, when learning gets under weigh, substantiates the correctness of this assumption.

6 is a plot of the reciprocals of these probable errors against the percentage of trials required to reach the end-point. The similarity between the graphs of Figures 3 and 6 is at once apparent. It is worth noting that if smooth curves were drawn through the points of Figures 4, 5, and 6 the resulting "learning curves" would have sigmoid characteristics, especially apparent at 27.1° and at 28.3°. Below 27.1° the lower part of the *"S"* is in evidence, while at 29.4° the upper part is conspicuous. These facts are suggestive, but their interpretation should await further experimentation.

If both 1/(time) and 1/(*P.E.* of mean time) are each directly proportional to a chosen measure of the underlying physiological process, one would expect them to give similar graphs when plotted as a function of the percentage of trials. A plot of 1/time against 1/*P.E.*, averaged for each fifth part of the experiments should, moreover, give a rectilinear function after, let us say, the first fifth of the experiment when learning gets well under weigh. Figure 7 is this plot. The linear relation beyond the first point is a striking illustration of the mutual consistency of the two measures of learning. The result meets criticisms which might be made on the grounds that the measurements depend too much on one's judgment according to arbitrary criteria and are therefore subject to subconscious bias. It is quite impossible to see how subconscious bias could determine a functional progression with time in the probable errors of over a thousand observations.

III

The experiments might be criticized on the grounds that temperature has affected a sensory process rather than a central one, rendering the stimulus more effective at the higher temperatures. For this reason the buzzer stimulus previously referred to in the description of the maze was used to introduce an entirely different excitatory process. Table 2 summarizes the results at two significant temperatures and compares them with those when the peppermint oil stimulus was employed. Table 2 also contains data showing that dilution of the peppermint oil by water to 1/15 of its concentration in the former experiments has no effect on the rate of learning. The three sets of results are equivalent both relatively and absolutely.

It might still be objected that temperature merely accelerates the sensory processes whereby the ant finds the hole in the partition

TABLE 2

°C.	Buzzer stimulus: No. of trials with probable errors. Basis of 10 with "penalties"	Buzzer stimulus: No. of ants	Peppermint oil stimulus (full strength) (Data from Table 1)	Peppermint oil stimulus (full strength): No. of ants	Peppermint oil stimulus (1/15 of full strength): No. of trials with probable errors. Basis of 10 with "penalties"	Peppermint oil stimulus (1/15 of full strength): No. of ants
25.0	24.6±1.4	9	25.6±1.2	19	24.5±0.2	2
29.4	13.9±0.9	5	12.8±0.8	10	13.0±0.3	4

separating the two halves of the "maze." That this is very unlikely is indicated by the following experiment. After learning is complete, i.e., after the elimination of "random" movements, the time elapsing between stimulation and solution of the "problem" is composed of two factors: the time of orientation toward the hole, characterized usually by quick turning or "waltzing" movements of the ant, and the time occupied in moving through the hole. If, *after* learning is achieved, the ant takes longer to locate the hole at low temperatures than at high temperatures, due perhaps to less effective reception of orienting determinants, one would expect the orientation time to be relatively longer at the low temperature than at the high temperature. Eight ants were taught the maze, four at 19.4° and four at 29.4°C. Their "orientation times" were measured and compared to the total time involved in the solution *after learning was complete.* Table 3 shows the results. Each figure is the average

TABLE 3
EFFECT OF TEMPERATURE ON ORIENTATION TIME

19.4°		29.4°	
Mean total time of solution after "learning" is complete	Orientation time	Mean total time of solution after "learning" is complete	Orientation time
2.7	1.2	2.7	2.1
4.0	2.9	4.9	4.1
3.1	1.7	1.8	1.3
2.1	0.9	3.0	1.8
Mean 3.0	1.7	3.1	2.3
$\frac{\text{Orientation time}}{\text{Total time}} = 0.56$		$\frac{\text{Orientation time}}{\text{Total time}} = 0.75$	

from approximately 10 correct solutions and the means of these means are shown. There is actually less time taken for orientation, absolutely *and* relatively, at the lower temperature. This is probably due to a general hyperexcitability of the ant at the higher temperature, which causes a prolongation of orientation time due to a greater initial "shocking" effect of the stimulus.

IV

The results on relearning are not so complete as those for learning, owing to rather high casualties when the ants were transferred from

TABLE 4
EFFECT OF TEMPERATURE ON RETENTION OF "LEARNING"
Criterion of 10 with "penalties"

	°C.					
	29.4*	28.3	27.1	25.0	19.4	15.0
Trials to learn						
———	0.62±0.05	0.94±03	1.14±0.11	1.23±04	1.54±09	1.36±1.2
Trials to relearn						
No. of ants	5	3	4	8	7	4

*The data at 29.4 are from ants of Colony 1.

maze to bottle and back again after 18 hours in the thermostat. Table 4 records the results. The ratio $\frac{\textit{trials to learn}}{\textit{trials to relearn after 18 hours}}$ may be regarded as a measure of retention. On this scale perfect retention $= \infty$, no retention $= 1$.

At 29.4° it was found that relearning was very difficult. Control ants, however, with no previous experience with the maze, showed great retardation in learning after an exposure of 18 hours at this temperature. Since at 28.3° and 29.4° the ratio is less than 1, we have a condition of less than "no retention," which is obviously not to be explained by the assumption of the mere destruction at elevated temperatures of a hypothetically deposited "learning" facilitating substance, although such an interpretation is not inconsistent with the decline in retention up to 27.1°. There would, therefore, appear to be some general "toxic" effect after abrupt and long exposures to temperatures above at least 28.3°, a factor acting in the opposite way to that of the facilitation following shorter exposures to these temperatures. The range of 25° to 30° is, however, a normal activity range for this species of ant. Miss Fielde (9) states that 25° to 30° is the optimal temperature for pupation of this species. It is possible that, in the gradual increase of temperature to the heat of summer, compensatory factors mitigate the effects observed after these exposures in the thermostat.

V

In the foregoing experiments three criteria of learning have been used, with mutually consistent results, which indicate the existence

of quantifiable underlying physiological phenomena. The reciprocals of the number of trials to reach an end-point under the procedure outlined is probably the most reliable of these measures, but the agreement between these measurements and those given by the reciprocals of the times for learning and by the reciprocals of the probable errors of the learning times is most striking.

The groups of control experiments present strong evidence that the phenomenon studied is a true conditioning due to central nervous activity. Variation in effectiveness of the odor stimulus with temperature is ruled out by obtaining identical results with the use of a tactual stimulus involving separate excitatory processes. Variations in effectiveness of reception of orienting determinants, producing a pseudo-learning effect, is made unlikely by showing that there is no decline in orientation time with decreased temperatures.

The lack of effect of temperature on learning below 25° C. cannot at present be specifically interpreted. Such a result might be expected if learning were determined by certain purely physical factors such as diffusion, molecular orientations, or permeability changes which might alter "synaptic resistances" and facilitate conduction over certain pathways. On the other hand, a similar lack of temperature effect might occur if the facilitation depended upon equilibrium in a system with low heat of reaction, or in other ways upon opposing effects oppositely influenced by temperature.

Above 25° the rough value of $\mu = 7300$ calories is suggestive of chemical facilitation and is not inconsistent with the notion of the deposition of an "excitatory" substance somewhere in the reflex arc after the manner proposed by Sherrington (18) to account for facilitation and other phenomena of reflex conduction. This mechanism is also not inconsistent with that proposed by Robertson to account for learning, although the foregoing experiments contribute nothing to Robertson's arguments for autocatalysis based on the sigmoid nature of the learning curve; these arguments in themselves can hardly be considered pertinent (11). Owing to possible toxic effects at 28.3° and 29.4° and to the possible complexities of the learning process, the value of μ of 7300 calories can, at present, be given little significance as a "catalyst indicator."

The experiments on retention are complicated by possible destructive effects above 28.3°. It is noteworthy that below 25° temperature seems to have no effect on the rate of "forgetting," while

above this temperature ants "forget" rapidly, although learning much faster if not exposed for too long periods. These facts again favor a chemical interpretation of the control of learning and of forgetting above 25°. Control experiments, however, at 29.4° on 10 ants that had been kept for 18 hours at this temperature and which then learned the maze for the first time, show that these ants took the same excessively large number of trials to learn as did the ants which relearned the maze after similar exposures. This fact makes interpretation of the results of the experiments on relearning somewhat equivocal.

Summary

The rate of conditioning of ants to a simple "maze" is measured as a function of temperature. The rate of learning is unaffected by temperatures from 15° to 25°, but is accelerated about 100% for the range of 25° to 29.4°, provided that the duration of exposure to the higher temperatures is not too long. Retention of learning after 18 hours is unaffected by temperatures below 25°. After exposures of 18 hours at 28.3° or 29.4° the ants not only "forget" the solution of the maze but take many more than the initial number of trials to relearn it. Great retardation of learning is also found in control ants exposed to these temperatures.

Methods of measurement of learning and theoretical interpretations of the temperature effects on learning and retention are discussed.

References

1. Crozier, W. J. On biological oxidations as function of temperature. *J. Gen. Physiol.*, 1924, **7**, 189-216.
2. ————. On the critical thermal increment for the locomotion of a diplopod. *J. Gen. Physiol.*, 1924, **7**, 123-136.
3. ————. Note on the distribution of critical temperatures for biological processes. *J. Gen. Physiol.*, 1926, **9**, 525-529.
4. ————. The study of living organisms. Chap. 2 in The foundations of experimental psychology. Worcester, Mass.: Clark Univ. Press, 1929. Pp. 45-127.
5. Crozier, W. J., & Federighi, H. Phototropic circus movements of *Limax* as affected by temperature. *J. Gen. Physiol.*, 1924, **7**, 151-169.
6. Crozier, W. J., & Stier, T. J. B. The temperature characteristic of pharyngeal breathing rhythm of the frog. *J. Gen. Physiol.*, 1924, **7**, 571-579.
7. ————. Temperature and frequency of cardiac contractions in embryos of *Limulus*. *J. Gen. Physiol.*, 1927, **10**, 501-518.

8. EBBINGHAUS, H. Memory: A contribution to experimental psychology. (Trans. by H. A. Ruger and C. E. Bussenius.) New York: Teachers Coll., Columbia Univ., 1913. Pp. 123.
9. FIELDE, A. M. Temperature as a factor in the development of ants. *Biol. Bull.*, 1905, **9**, 361-367.
10. HOAGLAND, H. The mechanism of tonic immobility (animal hypnosis). *J. Gen. Psychol.*, 1928, **1**, 427-447.
11. ————. The Weber-Fechner law and the all-or-none theory. *J. Gen. Psychol.*, 1930, **3**, 351-373.
12. HUNTER, W. S. Learning: II. Experimental studies of learning. Chap. 15 in The foundations of experimental psychology. Worcester, Mass.: Clark Univ. Press, 1929. Pp. 564-660.
13. LASHLEY, K. S. Learning: I. Nervous mechanisms in learning. Chap. 14 in The foundations of experimental psychology. Worcester, Mass.: Clark Univ. Press, 1929. Pp. 524-563.
14. PAVLOV, I. P. Conditioned reflexes: an investigation of the physiological activity of the cerebral cortex. Oxford: Oxford Univ. Press, 1927. Pp. xv+430.
15. ROBERTSON, T. B. The biological conception of memory and sensation. *Monist,* 1909, **19**, 367-386.
16. ————. Principles of biochemistry. Philadelphia: Lea and Febiger, 1920.
17. SHAPLEY, H. Thermokinetics of *Tiometropum apiculatum* Mayr. *Proc. Proc. Nat. Acad. Sci.*, 1920, **6**, 204-211.
18. SHERRINGTON, C. S. Remarks on some aspects of reflex inhibition. *Proc. Roy. Soc.*, 1925, **97B**, 519-545.

Harvard University
Cambridge, Massachusetts

UNE ÉTUDE DE LA PHYSIOLOGIE DE L'APPRENTISSAGE CHEZ LES FOURMIS

(Résumé)

On a mesuré comme fonction de température la vitesse de conditionner les fourmis à un simple "labyrinthe." La vitesse de l'apprentissage n'est pas influencée par les températures de 15° à 25° mais est accélérée environ 100% entre 25° à 29,04°, pourvu que la durée de l'exposition aux températures les plus élevées ne soit pas trop longue. La rétention de l'apprentissage après 18 heures n'est pas influencée par les températures au-dessous de 25°. Après des expositions de 18 heures à 28,03° ou à 29,04° les fourmis non seulement "oublient" la solution du labyrinthe mais suivent beaucoup plus que le premier nombre de parcours pour l'apprendre de nouveau. On trouve aussi une plus grande retardation de l'apprentissage chez les fourmis de contrôle exposées à ces températures.

On discute les méthodes de la mesure de l'apprentissage et les interprétations théoriques des effets de la température sur l'apprentissage et la rétention.

HOAGLAND

UNTERSUCHUNG ÜBER DIE PHYSIOLOGIE DES LERNENS BEI AMEISEN

(Referat)

Die Schnelligkeit, mit der sich Ameisen an ein einfaches Labyrinth ('maze') gewöhnen wird gemessen in ihrem Verhältniss zur Temperatur. Die Geschwindigkeit des Lernens wird durch Temperaturen von 15° bis 25° nicht beeinflusst, wird aber im Umfang 25° bis 29.4° ungefähr mit 100% beschleunigt, wenn die Blossstellung den höheren Temperaturen gegenüber nicht zu lange dauert. Die Beibehaltung des Gelernten wird durch Temperaturen unter 25° nicht beeinflusst. Nach Blossstellungen bei 28.3° oder 29.4° achzehn Stunden dauerend haben die Ameisen nicht nur die Lösung des Labyrinths vergessen; sie brauchen nun viel mehr als die anfängliche Fährtenzahl um es wieder zu lernen. Grosse Verzögerung des Lernens wird auch bei diesen Temperaturen ausgesetzten Kontrollameisen gefunden.

Methoden zur Messung des Lernens und theoretische Deutungen der Temperaturwirkungen auf Lernen und Beibehalten werden besprochen.

HOAGLAND

CONDITIONED REFLEX AND PERCEPTION: THE CONCEPT OF SYNCRETISM

From the Institut de Psychologie, Université de Paris

HENRI PIÉRON

By means of the establishment of conditioned reflexes it is possible to study objectively the perceptions of animals, and much of the research done in Pavlov's laboratory has been devoted to those characteristics which he himself calls the "sensory analyzers."

A study of the perceptive functions makes evident those processes which are superimposed upon those entering into the development of a conditioned reflex. As Pavlov (1, pp. 100-101; 2, p. 189) has so aptly noted: "What Helmholtz has designated by the well-known term, 'unconscious cerebration,' is really nothing more than the mechanism of the conditioned reflex. For example, when a physiologist is convinced that for the perception of the dimensions of an object one must have both an image of a certain size on the retina and a definite adjustment of the intrinsic and extrinsic eye muscles, he recognizes in this fact the mechanism of the conditioned reflex. The group of retinal and muscular excitations, having coincided a number of times with the tactual excitation produced by the given dimensions of the object, becomes a sign, a conditioned excitant, of the actual dimensions of an object. Granted this point, it is clear that the facts constituting the psychological part of physiological optics are, from the physiological point of view, nothing more than a series of conditioned reflexes, or simply the elementary facts of the optic analyzer."

The concept of the conditioned reflex, introduced into physiology by Pavlov, links up with the idea of anticipation to which certain observations on animals had led and of which Pavlov was totally unaware until several years after he had directed his laboratory into this new line of research. At the same time that the great Russian movement in physiology began, I was also struck with the importance of the idea of anticipation for the explanation of the adaptive reactions of animals, and especially of the acquisition of certain rhythms, and I introduced the concept of the mechanism of the "anticipation reflex" (3, 4), which is no different from that of the conditioned reflex.

Perceptive reactions consist principally of adaptive reactions, and we see elements habitually linked to a complex experience substituting themselves for the total experience of which they are the "sign," to use Pavlov's terminology, or of which they are the symbol, as one might also say; for symbolism, which plays such a considerable rôle in the intellectual development of humanity, has its base or biological foundation in this process of conditioned anticipation (5).

Thus one may regard the study of the laws of perception and the study of conditioned mechanisms as ultimately having a common object or, at least, as having an important community of aim.

Pavlov's statements with regard to the conditioned mechanisms are in full accord with the classic conceptions of the genesis of perception. However, recent investigations of the psychology of perception have shown that the classic conceptions were not at all satisfactory; and since then the concept of perception and that of the conditioned reflex seem to have been entirely divorced from each other.

We wish to raise the question, however, as to whether, in our conception of the conditioned mechanism, we should not make some changes in the light of facts brought out by the study of perception, and thus take advantage of the closeness of the relationship between the different disciplines of biological science.

It is evident that between the structural conception of *Gestaltpsychologie,* defended by Köhler, and the genetic conception involved in learning and in the conditioned reflex there is an irreconcilable difference. However, the structural conception rests on a theory, which goes far beyond the facts and which must leave out a great number of positive data, which, in effect, involve the evolution of perceptive data and their correlative genesis of repeated experiences.

The facts which have been emphasized or discovered by the numerous Gestalt psychologists, those brought out by other recent experimental data, and even those found in the experimental accounts of psychologists accepting older theories, all agree in the rejection of the atomistic idea of perception as proceeding from elementary sensations and forming more and more complex systems as a sort of progressive synthesis. The complex is primitive and it is by means of analysis that the elements separate out from a syncretic totality—from an undifferentiated and chaotic *Ganzheit.* Studies of children as well as those of animals agree on the idea of initial syncretism.

If we note the statements of Pavlov on initial reactions in the process of setting up a conditioned reflex, we arrive at the same con-

clusion: that analysis is obtained by systematic and directed training. "In the formation of a conditioned reflex," according to Pavlov, "a definite analyzer comes into play by means of its general and relatively gross activity, and it is not until later, by means of progressive differentiation, that the simplest part of its activity becomes the conditioned excitant. If, for example, a luminous object is placed before a dog, at first the bright light is the excitant, and only gradually does the object act by itself" (1, p. 102).

Pavlov recognizes the initial existence of a certain undifferentiated syncretism which makes very important the chance modifications of the environment (from whence comes the necessity of establishing experimental laboratories which are protected from all extraneous and variable influences). The term "conditioned reflex to the environment" is an expression of this fundamental fact. "But later on," Pavlov writes, "when the special reflex to a single definite and constant stimulus has appeared, all the other elements of the environment gradually lose their special conditioned significance, most probably on account of a gradual development of internal inhibition" (2, p. 107).

However, for Pavlov, progressive analysis represents the simple effect of cerebral activity, the brain being essentially an analytic mechanism. It is at this point that the divergence between Pavlov's conceptions and those of modern studies of perception is most manifest. Perceptive analysis which permits of dissociation first of the primitive undifferentiated group of definite objects, then of the qualities of these objects (color, form, texture, etc.), then of the more elementary data (such as brightness and tint, etc.), is not accomplished spontaneously but is the product of training, especially when that training is systematically directed. It is only by artificial training that one can make an animal react to a particular tint or brightness independently of the nature and form of the objects of minute environmental differences. With regard to this, one might refer back to the experiments of Köhler and to his work with different species of birds and mammals.

Pavlov noted that similar difficulties are encountered in attempting to set up a response to a sensory quality by progressive differentiation. The procedure is one of constant reinforcement of the chosen quality by association with powdered meat, and of consistent presentation in the absence of this association of the sensory facts which are to be distinguished, always in the midst of environmental conditions

which have been rendered constant and whose ineffectiveness has been shown by continual repetition. However, Pavlov would not attribute the essential rôle to the cerebral processes regulated by the elementary sensations considered separately, in the sense that in the original complex these elements retain their entire individuality and act alone.

In this account of the facts let us examine the way in which this interpretation of the mechanism is presented and the concepts which it involves.

Let us take the example of a certain light stimulus (an electric light in a dark room) presented in a definite environmental complex which is held rigorously constant. Each time the light appears, and only at such times, a dog is given some powdered meat which causes him to salivate. The light, by conditioned association, acquires the ability to produce reflexes and to excite salivation. Let us present from time to time, in addition to the light, the sound of a tuning fork, in this case never giving the powdered meat. Salivation, which is slightly less from the beginning, continues to diminish progressively and finally disappears. What has happened? The new stimulus, which originally had a certain feeble inhibitory capacity, acquires, in the train of repetitions without the reinforcement, considerable inhibitory power which prevails over the excitatory capacity of the light and prevents salivation. There is a conflict between a conditioned excitation and a conditioned inhibition, each bound to a specific stimulus, respective forces of which are measurable in terms of the quantitative evaluation of the salivary results.

However, let us present in addition to the light and the sound of the tuning fork the sound of a metronome, and in thus doing, let us constantly give powdered meat to the dog whenever the metronome is utilized. Gradually, salivation reappears, and, after a sufficient number of associations, will be quantitatively equal to that called forth by the light alone if we continue to associate the powdered meat with this last stimulus in order to preserve its reflex power.

Now to the atomistic interpretation: there are three forces which come into play, the excitatory force of the light, the inhibitory force of the tuning fork which combats the first, and a third force, "disinhibitory," which combats the second and thus liberates the first to such a degree that the cancellation of the second becomes more complete.

The principle of inhibition of an inhibition, of the checking of a check, introduced by Pavlov, may be extended elsewhere. A new

stimulus added, in the absence of conditioned reinforcement, will possess a fourth force which will oppose the third by cancelling it, and liberate the inhibitory action of the second, which in turn will check the inhibitor of the check, and so forth. There is here an idea to which Pavlov was naturally led, not without some reluctance, and which does not cease to appear rather strange, especially when one develops it to its natural consequences.

Now, we shall see that regarding this point in the arsenal of facts established by Pavlov's school itself, there are some crucial experiments which oppose an atomistic interpretation and agree, on the contrary, with a different interpretation based on the facts of perception. For this reason, we shall discuss in detail some experiments which were carried out by P. N. Nicolaïev and are described by Pavlov (1, pp. 78-87).

The light stimulus (L) after repeated associations with the powdered meat calls forth the maximum salivary effect (10 drops in 30 seconds). The sound of the tuning fork (S) is then given with the light without adding the powdered meat: salivation remains maximum, and, after two or three repetitions of the combination, the sound alone has acquired by transfer a certain stimulus value for producing salivation, giving one or two drops of saliva the first time that it is used, but no more from the second time on; and the combination repeated without reinforcement gradually loses its stimulus value, giving 8 drops, 5, then 4, and 3, and finally 0. At this same time, the light stimulus alone still produces its 10 drops.

Thus: $L=10$ $L+S=0$

One then adds to the combination $L+S$ the rhythmic sound of the metronome (M). The trio $L+S+M$ does not produce salivation; however, when this combination is repeated with constant association of powdered meat, it acquires a growing stimulus value calling forth secretions of from 2 to 4 drops of saliva. At the same time, L alone still produces 10 drops, but if the metronome is presented with it, it does not produce more than 6 drops.

Thus we have:

$L=10$	$L+M=6$	$L+S+M=4$
$S=0$	$M=0$	$S+M=0$
		$L+S=0$

Thus we see the double action of the metronome. In connection with S, inhibitor of L, M is a disinhibitor which checks inhibition; but, in connection with L, it plays the rôle of inhibitor even though it is the antagonist of S in the situation $L+S+M$.

Pavlov is obliged to admit that M is an inhibitor of a general sort, its inhibitory action affecting at the same time the excitatory action of L and the inhibitory action of S, but, "falling in the inhibited area, cancels only the inhibitor, thus liberating from that influence the conditioned excitant." This interpretation is not absolutely satisfactory.[1]

Let us follow the same experiment: we continue to present the powdered meat with the trio $L+S+M$, and the maximum salivary action is produced; at this same time the very excitatory action of the metronome alone, which had manifested itself earlier in an undeveloped form, becomes distinct and there no longer continues to be any inhibition of L by M.

Thus we have:	$L=10$	$L+M=10$	$L+S+M=10$
	$M=4$	$S=0$	$L+S=0$

At this time, we have a strong conditioned excitant (L), a conditioned inhibitor (S), and a weak conditioned excitant (M), the disinhibitor of S.

If S and M are presented simultaneously, what occurs? Since M alone produces 4 drops of saliva, when it is in conflict with S, a strong inhibitor, its normal action should be diminished. Now, that is not the case: $S+M$ give 4 drops just as M alone does. According to Pavlov, the part which the sound plays is very interesting, since "it acts differently in different combinations." That bears out the hypothesis that "there exists a sort of nervous equilibrium, the exact nature of which cannot be determined."

Is it possible, then, to continue to consider as definite the properties of these elementary constituents when these very properties change and become inverted within the complexes to which they belong? In some cases, Pavlov is, moreover, led to give to a complex a certain value with regard to the combination of the same elements when given in varied order: the series [*whistling, high tone, low tone, sound of a bell*] is, for example, excitatory, while the series [*whistling, low tone, high tone, sound of a bell*] is inhibitory by simple interchange of two elements; this is not obtained, moreover, except by a

[1]Certainly it is to be admitted that an ordinary excitant exercises, with conditions of preliminary equilibrium, opposing influences, just as an emotion calls forth movement of the intestines if the latter is in a state of rest or stops it if in movement, and Pavlov has noted that a strange and new stimulus can increase salivation to a certain point and diminish it to another (2, p. 90), but these ordinary actions are not to be confused with the specific influence of conditioned excitants or inhibitors.

long series of differentiations. "How can we interpret these facts?" says Pavlov. "In what manner does a complex composed of the same excitants and acting consequently on the same cells of the hemispheres provoke in most chromatic fashion during the same period now an excitatory process, now an inhibitory process? On what can this difference in action be based? As far as facts show, the basis of this cannot be explained except by resorting to synthesis of the activity of the excited cells. In these particular circumstances, there must be some connections between the cells forming a complex unity such as we constantly observe in the formation of conditioned reflexes. This being the case, there must also exist the influence of the cells one upon the other as is so plainly manifest in the simultaneous complex of excitants" (2, p. 135-136). Pavlov then, it seems, is led to admit that the properties of the whole are different from those of the parts; because of this, without renouncing a certain type of analysis, would it not be better as a point of departure to substitute for the idea of properties of elements the idea of properties of various perceptive complexes?

Although, to my knowledge at least, the experiment has not been done for conditioned reflexes, I can, basing my claim on various learning experiments, affirm that with sufficient repetition of the positive trials of the association of powdered meat with the complex $L+M+S$, and also with sufficient repetition of the negative trials not involving the association of meat but concerned with isolated elements L, M, and S, or with the combinations, $L+M$, $M+S$, $L+S$, one should be able to obtain salivation for the triple combination, since the double combinations will act as inhibitors just as the isolated elements did; the complete grouping will possess properties which are the inverse of all the constituent isolated elements, or which will be reunited in smaller groupings.

One cannot, therefore, understand the influence of the whole by consideration of the elements of which it would seem to be the synthesis.

Actually, in utilizing a light stimulus, we have, where environmental conditions are supposedly invariable, a certain perceptual complex which might be represented by the symbol $[L+X]$, uniting under the letter X the whole group of environmental conditions which are added to the light and which intervene strongly enough in the beginning, even in the absence of L, to call forth a certain salivary action which Pavlov mentions when he speaks of the "reflex to the environ-

ment." By repetition of the association of powdered meat with [$L+X$], when X alone is never thus associated, the perceptive complex [$L+X$] acquires value as a precursory sign of the powdered meat, and is reinforced every time the meat is presented, whereas failure to present the meat with X removes this value from the environmental complex X.

If we now introduce the new complex [$L+S+X$] and fail to present the meat each time, this complex, similar to [$L+S$], which, in virtue of the laws of analogy valid for perception as well as conditioned reflexes, at first seemed to be a probable sign of the meat, now finds itself deprived of this sign value as a consequence of the course of experience.

The sound, considered apart from the light, as long as it has never been used in experimentation, inevitably acquires an analogous value; the resemblance of [$S+X$] with X and with [$L+S+X$] (the two complexes having been presented in experimentation without the powdered meat) tends to give the complex [$S+X$] a negative value which is reinforced by repeating [$S+X$] without the powdered meat; however, if [$S+X$] is repeated in association with the meat, it becomes a conditioned stimulus (whereas [$L+S+X$] remains ineffective in proportion to the degree to which one prevents all analogous transfer by repetition of the trials without the presentation of meat).

Let us add another term, M; we thus have a new complex [$L+S+M+X$], which, by analogy, will act in the beginning like [$L+S+X$] to which it is most nearly related, and will not, therefore, produce salivation, but which, by association with the meat, will acquire value as a conditioned stimulus, not because M exercises any particular influence, but because the perceptive complex of which it is an integral part has made certain by a series of successes its precursory and symbolic value with regard to the meat.

The analogy of the positive complexes [$L+X$] and [$L+S+M+X$] assures positive value to the complex [$L+M+X$] not previously experimented with—a complex to which experimentation could later give an opposed negative value or else a confirmation of its positive value.

This symbolic value belongs to the complexes, and not to the elements separately considered; and a new complex will assume a more or less distinct value as a function of the value of the most closely related complexes. Let us assume the following potent complexes. By analogy with the preceding, the complex [$M+X$] has a certain weak value, whereas [$S+X$] and [$L+S+X$] are negative.

When experimenting with the new complex $[S+M+X]$ for the first time, one discovers that it has a certain weak value exactly like that of $[M+X]$. This is due to the fact that the value of the one as well as of the other is derived, analogously, from the same potent complex $[L+S+M+X]$.

In order to understand these facts, there is no need of insisting upon a nervous equilibrium of some unknown nature, and it seems that one can predict with certainty the relative potency of new complexes.

The apparently different rôle of the sensory units in the various artificial complexes appears simply to support the fact that the true rôle does not belong to them, but, as in the case of the natural complexes of perception, belongs only to wholes.

Analysis is certainly possible, as in perception. One can, for example, give L a positive value in all possible combinations ($L+X$, $L+Y$, $L+Z$, etc.). One can even give this value to a particular aspect of L, L', or L'' (tint, brightness, etc.), but only after an extraordinarily great number of trials which will justify anticipation of success each time L is present in the combinations presented, but which will afford only disappointment every time that L is omitted. One can force L to acquire value in itself, but only by imposing upon the nervous system of the organism an analytical differentiation which is not at all the result of the play of its natural functions and tendencies, although the fundamental processes are always limited to excitation and inhibition (with no call here, either, for "inhibition of inhibition").

From this point of view, we see the agreement between the processes of perception and the mechanisms involved in conditioned reflexes, and the two major laws which I have deduced from the study of perceptive phenomena seem to apply directly to the phenomena studied by Pavlov: "(1) A psycho-physiological complex which has had some connection with a given objective experience (permitting a success of adaptive reactions) arouses the specific perceptive reaction of this experience, and does this the more effectively the more frequent the perception. (2) A new psycho-physiological complex tends to arouse a specific perceptive reaction which has had some connection with a similar complex which has been previously experienced, and does this the more effectively the more frequent the relation has been and the more marked the correspondence is" (5, p. 22).

These laws constitute the basis for predicting the operation of the artificial complexes used by Pavlov in the study of reflexes, once one

knows the experiences to which the animal has previously been subjected. The organism—and it is this which makes the study of its various mechanisms difficult—is a machine which manifests a strict solidarity of all its parts, and this unity arises, for the most part, from the nervous system, which possesses in the highest degree the characteristic of functional interdependence.

The behavior of the animal is not the function of an isolated stimulation of some cortical receptor-neuron, but of a whole series of simultaneous and immediately preceding stimulations, the function being determined by the integration of all of its past experiences. Theoretically, one is able to isolate a stimulation from a totality, but for that reason it does not necessarily mean that it will act in an isolated manner in the organism studied; it is only by prolonged experimental reinforcement that we can give to this stimulus, in certain definite circumstances (on condition that there are not too many important variations of internal origin in the organism, no toxic actions, no fatigue, etc.), a preponderance over all the rest of the perceptive complexes sufficiently great to permit it to act as though it were alone and thus to appear abstracted by analysis. But the preponderance does not completely disrupt the unitary coherence and does not in any case permit us to return to atomistic terminology in explaining nervous mechanisms.

Upon this point, physiology and psychology ought to be completely in accord, since they constitute two points of view in the study of organisms, in biological science, and it matters little whether one speak in terms of reflexes or of perceptions when one deals with processes which are fundamentally the same.

References

1. Pavlov, I. P. Les sciences naturelles et le cerveau. (XII Congrès des Savants et Médecins russes, Moscow, December 28, 1909). In *Les reflexes conditionnels.* (Trans. by Gricouroff.) Paris: Alcan, 1927. Pp. 379.
2. ————. Leçons sur l'activité du cortex cerebral. (Trans. by M. I. Trifonoff.) Paris: Legrand, 1929. Pp. 418.
3. Piéron, H. La réaction aux marées par anticipation réflèxe chez *l'Actinia equina. C. r. Soc. de biol.,* 1906, **61**, 658-660.
4. ———— Des phénomènes d'adaptation biologique par anticipation rythmique. *C. r. Acad. des sci.,* 1907, **144**, 338-341.
5. ————. Les problèms de la perception et la psycho-physiologie. *Année psychol,* 1927, **27**, 1-22.

Université de Paris
Paris, France

LE RÉFLEXE CONDITIONNEL ET LA PERCEPTION: LE CONCEPT DU SYNCRÉTISME

(Résumé)

Le but de cet article est d'indiquer les similarités qui existent entre le concept moderne des processus perceptifs et les mécanismes du réflexe conditionnel. L'évidence expérimentale récente s'oppose à l'ancien concept de la nature atomistique du processus perceptif, mais semble indiquer un syncrétisme initial ou totalité syncrétiste dont les éléments sont séparés par un processus analytique. Ce concept s'accorde avec les résultats de Pavlov dans ses études du réflexe conditionnel. Les réactions à une situation de stimulus sont d'abord à la situation totale, et l'analyse, la réaction à une partie spécifique de la situation, est obtenue seulement par l'entraînement systématique et dirigé. L'auteur examine de divers aspects des mécanismes et des concepts du réflexe conditionnel et les trouve en accord avec le concept de l'auteur des processus de la perception et les deux lois importantes proposées par lui: (*a*) "Un complexe psycho-physiologique qui a quelques rapports avec une expérience objective donnée (permettant un succès des réactions adaptables) fait naître la réaction perceptive spécifique de cette expérience, et plus la perception est fréquente, plus ceci a lieu efficacement." (b) "Un nouveau complexe psycho-physiologique tend à faire naître une réaction perceptive spécifique qui a quelques rapports avec un complexe pareil éprouvé auparavant, et plus la réaction a été fréquente et plus la correspondence est marquée, plus ceci a lieu efficacement." PIÉRON

BEDINGTER REFLEX UND WAHRNEHMUNG: DER BEGRIFF DER GLEICHZEITIGKEIT

(Referat)

Es ist die Absicht dieser Arbeit zu zeigen, dass Ähnlichkeiten bestehen zwischen der modernen Auffassung über Wahrnehmungsprozesse und die Mechanismen der bedingten Reflexe. Experimentelle Beweise der letzten Zeit sind der älteren Auffassung atomischer Natur über die Wahrnehmungsprozesse entgegengesetzt, scheinen aber eine anfängliche Gleichzeitigkeit oder ein gleichzeitiges Ganzes anzuzeigen, aus dem heraus Elemente durch einen analytischen Prozess ausgeschieden werden. Diese Auffassung stimmt überein mit den Ergebnissen Pavlovs in seinen Untersuchungen über die bedingten Reflexe. Die Reaktionen auf eine Reizsituation sind zunächst die einer Gesamtsituation, und eine Analyse, die Reaktion auf einen spezifischen Teil der Situation, wird nur durch eine systematische und direkte Übung erworben. Man prüfte verschiedene Gesichtspunkte der Mechanismen und der Aufassungen über den bedingten Reflex, und fand dabei, dass sie mit der Auffassung des Autoren über den Prozess der Wahrnehmung und den von ihm vorgeschlagenen zwei Hauptgesetzen übereinstimmen:—a) "Ein psychophysiologischer Komplex, der eine Beziehung hatte zu einer gegebenen, objektiven Erfahrung (die einen Erfolg der anpassenden Reaktion zulässt) ruft eine spezifische Wahrnehmungsreaktion dieser Erfahrung hervor, und tut das um so wirksamer, je häufiger die Wahrnehmung vollzogen wird." b) "Ein neuer psychophysiologischer Komplex hat die Tendenz eine spezifische Wahrnehmungsreaktion hervorzurufen, die zuvor eine Beziehung zu einem ähnlichen Komplex hatte, der früher erlebt wurde, und tut dies um so wirksamer, je häufiger die Beziehung erfolgte und je deutlicher die Übereinstimmung (correspondance) war. PIÉRON

THE LABORATORY COURSE IN PSYCHOLOGY

I. Methods Employed for Ranking Stimuli*

From the Psychological Laboratories of the University of Minnesota

Miles A. Tinker

In certain experiments in the psychological laboratory, as well as in many experimental investigations, it is necessary to obtain a relative estimate of the excellence or value of the stimuli in a series. The earlier texts of experimental psychology employed the method of paired comparisons to arrive at these relative estimates. Foster's book (7), published in 1923, makes use of the order of merit method as well as the method of paired comparisons. In the recent revision of Foster's text (8) the graphic rating scale as well as the two methods cited above are employed to obtain relative estimates of excellence.

In the method of paired comparisons every specimen in a stimulus series is compared directly and individually with every other member of the series. The stimuli are presented in pairs and a comparative judgment called for. The order of merit method involves arranging the members of a series according to some designated order. With the graphic rating scale the excellence of a trait or value is estimated by checking along a horizontal line. This checking is guided by descriptive phrases representing the distribution of the trait from maximum to minimum (4, pp. 317-370).

The method of paired comparisons requires more time and is apt to be more tedious to both the experimenter and to the subject than either the order of merit method or the graphic rating method. In certain laboratory experiments it is more convenient or desirable to use one method in preference to others. If the above-mentioned methods yield approximately the same rankings of stimuli, the method most readily applicable in a given laboratory situation may be employed and satisfactory results obtained. It is desirable, therefore, to possess knowledge concerning the equivalence or lack of equivalence of these methods in various experimental situations.

*Recommended for publication by Calvin P. Stone, accepted by Carl Murchison of the Editorial Board, and received in the Editorial Office, March 4, 1930.

The purpose of this study is (*a*) to evaluate, by an analysis of the literature, the use of various methods for obtaining relative estimates of excellence, and (*b*) to present further experimental results comparing the following methods: paired comparisons, order of merit, and absolute rating on a graphic scale.

Analysis of Previous Studies

The method of paired comparisons was used in psychology during the latter part of the nineteenth century. This method is still employed extensively both in laboratory classes and in research. Cattell (5) was the first to employ the order of merit method in an experiment reported in 1902. Since then Cattell, Thorndike, Hollingworth, Strong,[1] and others have made extensive use of the latter method in a variety of experimental investigations. The order of merit method is considered by some to be less "accurate" than the method of paired comparisons[2] because of the fact that time and space errors can be controlled in the latter and not in the former. Strong (11), however, has shown that the order of merit method is valid for estimating the relative value of advertisements. Obvious advantages of the order of merit method are (*a*) that it is quicker and easier to use, and (*b*) that it is less tedious to both experimenter and subject than the method of paired comparisons. The use of the former facilitates the gathering of data from a large number of subjects. Strong (11) has pointed out that the order of merit method may be employed satisfactorily with either simple or complex stimuli. The graphic rating scale possesses all the advantages cited for the order of merit method. Moreover, it is readily adapted to group testing, and rating on a graphic scale is practically always interesting to the subjects.

Comparisons of results obtained by different methods of ranking stimuli have been made in various studies. A comparison of the order of merit method and the method of paired comparisons is reported by Barrett (1). Three types of judgments by ten subjects were involved in the investigation: (*a*) a series of 15 weights which were considered to yield very objective judgments; (*b*) 15 specimens of handwriting, for semi-subjective judgments; and (*c*) 15 propo-

[1]For bibliographies and brief reviews of methods see Strong (11) and Barrett (1).

[2]See Foster (7), p. 235.

sitions to test degree of belief, for very subjective judgments. The results obtained by the use of the two methods showed a correlation of .989 for the series of weights, .997 for handwriting, and .979 for beliefs. These yield an average correlation of .987. The validity coefficient for judgments of weights was .997 by the order of merit method and .997 for paired comparisons; for handwriting, .997 and 1.00, respectively. The two methods appear to yield identical results and to be equally valid in these situations.

In 1925 Brentlinger (3) made a study of judgment by the method of paired comparisons versus the order of merit method in which 19 advertisements were rated for familiarity by seven subjects. Correlation of the rankings yielded by the two methods gave a coefficient of .964, indicating that these methods yield approximately equivalent results in this situation. Furthermore, the ratings were as reliable in one method as in the other (both highly reliable).

More recently, Hevner (10), in a comparative study of three psychophysical methods in which 370 subjects were used, reports a rigid scientific comparison of the obtained results when specimens of handwriting were judged for relative excellence by the method of paired comparisons, the order of merit method, and the method of equal-appearing intervals. Twenty specimens of handwriting were employed in the first two and 72 (including the 20) in the last method. In the method of equal-appearing intervals the 72 specimens were sorted into 11 piles arranged in steps of successive excellence. Comparison of the methods with each other demonstrated that the method of paired comparisons and the order of merit method yield equivalent results in this experimental situation but that the method of equal-appearing intervals is not equivalent to either of the other two. The reliability and validity of the paired comparisons method and the order of merit method was demonstrated, but the method of equal-appearing intervals was not evaluated for either validity or reliability.

It is obvious that there are some situations where certain methods of ranking stimuli cannot be employed, i.e., with auditory stimuli the order of merit method cannot be employed. It is also reasonable to suppose that there are materials of such character that a certain kind of judgment is better obtained by one method than by others, or that the ranking of the series of stimuli by one method is not equivalent to that by another method. Gordon (9), for instance,

states that there is a serious objection against employing the method of paired comparisons for esthetic tests because presenting the combinations "so quickly exhausts the esthetic reaction."

In studying the influence of color on apparent weight, DeCamp (6) noted that the results obtained by the method of paired comparisons were not in harmony with those yielded by the order of merit procedure. He favors the first method because "the former appears to be the more scientific method."[3] In an experiment on visual perception versus visual plus kinaesthetic perception in judging colored weights, Taylor (12) discovered slight agreement between results in the order of merit method and in the method of paired comparisons. The correlation coefficients between methods in the two parts of the experiment were respectively .658 (reliability of P.C. method, .947; of O.M. method, .903) and .329 (reliability of P.C. method, .302; of O.M. method, .764). If we correct the intercorrelation coefficients for attenuation they become .716 and .684, respectively. These latter coefficients indicate that the two methods are far from equivalent in this situation.

The above analysis establishes the fact that the order of merit method of ranking stimuli is equivalent to the method of paired comparisons for obtaining relative judgments of (*a*) weights, (*b*) excellence of handwriting, (*c*) beliefs, and (*d*) memory value of advertisements. The methods are not equivalent for judging the relative weights of colored objects. Since any two methods may yield equivalent results in one situation but not in others, further comparisons of different methods for estimating the relative excellence of stimuli in various experimental situations are needed.

Apparatus and Procedure

There are two main parts to this experiment. In Part I, 10 stimuli (eight colors plus black and gray) were ranked for affective value by each of two variations of the order of merit method, by the method of paired comparisons, and by a graphic rating procedure. Results from the various methods were compared. In Part II, 12 musical intervals were ranked for affective value by the method of

[3]Other experimental findings do not justify this criticism. For those who wish to retain a part of the paired comparisons procedure and still secure the advantages of the order of merit method, a modification of the order of merit method given in Foster and Tinker (8, pp. 296-297) may be followed.

paired comparisons and by the graphic rating method used in Part I. Comparisons were made between the two methods.

Part I. The stimulus material consisted of 10 sheets of colored paper (eight Milton Bradley colors, classified as "standard" plus black and dark, neutral gray), 4 by 6 inches, mounted on white cardboard, 8 by 12 inches. Each sheet of cardboard carried a large number printed below the colored paper. The stimuli used were red, green, blue, yellow, orange, violet, yellow-green, blue-green, black, and dark, neutral gray. Numbers instead of names were employed in the responses. Thirty university students served as subjects. They observed in small groups or individually. Usually a period of a few days intervened between the responses in one method and those in other methods.[4]

In Method I (order of merit) the series of stimulus cards were tacked on the wall and the subjects were asked to rank the stimuli in serial order from most to least preferred. The number of the most preferred stimulus was to be written opposite rank 1, of the least preferred opposite rank 10. For the other variation of the order of merit method (Method II) the subjects were asked to arrange the specimens in order of preference on a table with the best-liked stimulus at the left and the least preferred at the right. In the method of paired comparisons (Method III) the experiment was conducted in the usual manner. Each pair of stimuli was exposed for three seconds. A graphic rating scale was employed in Method IV. There was a horizontal line for each specimen and underneath the line, for guidance of the rater, were the phrases: very displeasing, somewhat displeasing, neither pleasing nor displeasing, somewhat pleasing, very pleasing. Degree of liking was indicated by placing an X at an appropriate place on the scale. Only one stimulus was exposed at a time. The score for any specimen was the distance in millimeters of the X from the left end of the horizontal line.

[4]If subjects tend to remember the rankings assigned to the stimuli in any given method the correlations between methods would be raised. In our study 3 to 7 days intervened between responses in one method and those in any other method, with the exception that the method of paired comparisons and the order of merit method were given to a few subjects on the same day. The close agreement of our results with those of Barrett (1), who used a much longer interval between responses in different methods, would seem to indicate that memory effects have little influence in this experiment.

Part II. The stimuli consisted of the following musical intervals: octave, major seventh, minor seventh, major sixth, minor sixth, fifth, tritone, fourth, major third, minor third, major second, minor second. University sophomores served as subjects. There were two groups of these subjects consisting of 16 and 20 students, respectively. The intervals were played on an Ellis Harmonical and the responses made, first in the method of paired comparisons, and then by the graphic rating procedure. The graphic rating scale employed is the same as used in Part 1. Each interval was rated three times on the graphic scale and the average of the three constituted an individual's score for that interval. Collecting these data in Part II was a regular assignment in the psychology laboratory. The procedure in detail is given in Foster and Tinker (8, pp. 312-315).

Results and Discussion

Part I. The final order of stimuli as ranked, from most to least preferred, by the order of merit methods (Methods I and II) was determined by averaging the individual rankings for each of the 10 specimens. Using data from the method of paired comparisons (Method III), a final order of preference was obtained by ranking the total numbers of preferences for each specimen. Similarly, a final order of preference was determined from the data yielded by the graphic rating scale (Method IV) by ranking the sums of rating scores given each specimen.

The reliability coefficient of each method was computed by correlating the ranking of the stimuli from one-half of the group of subjects with the ranking from the other half and then applying the Brown "prophecy" formula to obtain the self-correlation for the whole group. The reliability coefficients follow: (Product-moment r is derived from the rank-order ρ by $r = 1.0233\rho$):

I. Order of merit (colors on wall); $r = .899$, $r_x = .946$.
II. Order of merit (arranging colors); $r = .908$, $r_x = .952$.
III. Paired comparisons; $r = .804$, $r_x = .891$.
IV. Graphic rating; $r = .707$, $r_x = .828$.

These coefficients show that the two variations of the order of merit method have very high and approximately equal reliability; that the method of paired comparisons is somewhat lower but still high enough for individual comparisons; and that, while the graphic rat-

ing has good reliability, it is somewhat less reliable than the other methods.

Table 1 gives a comparison of the methods with each other in terms of the raw coefficients and corrected (for attenuation) coefficients. In the first column are given the various comparisons. The

TABLE 1

INTERCORRELATIONS BETWEEN METHODS

Showing raw coefficients and coefficients corrected for attenuation. (Raw product-moment r derived from rank order ρ by $r=1.0233\rho$.)

Methods correlated	Raw r	r, corrected for attenuation	*P.E.* of corrected r
O.M. (I) and O.M. (II)	.927	.977	.010
O.M. (I) and P.C. (III)	.967	1.053	.008
O.M. (I) and G.R. (IV)	.890	1.032	.026
O.M. (II) and P.C. (III)	.967	1.049	.018
O.M. (II) and G.R. (IV)	.874	.984	.028
P.C. (III) and G.R. (IV)	.835	.973	.031

numerals (I, II, etc.) refer to the different methods as described above. (O.M. = order of merit; P.C. = paired comparisons; and G.R. = graphic rating). Examination of Column 2 reveals that the two order of merit methods and the method of paired comparisons correlate highly with each other, the coefficients ranging from .927 to .967. The fact that Method I, the order of merit procedure in which the specimens were tacked to the wall, agrees so well with Method II (O.M.) indicates that either variation of the order of merit method may be employed in this experiment and approximately identical results obtained. Because both Method I (O.M.) and II (O.M.) correlate very highly (.967) with Method III (P.C.), the three methods are approximately equivalent in this experimental situation. When the graphic rating method is correlated with the two variations of the O.M. method and with the P.C. method, the coefficients range from .835 to .890, showing marked agreement between graphic rating and the other methods. The lower reliability of the rating scale evidently affects these intercorrelations. In Column 3 are shown the r's corrected for attenuation, and in Column 4 the probable errors of the r's. Note that all the corrected coefficients are approximately 1.00. The four methods, therefore, correlate almost perfectly with each other within the limits of the reliability of each.

From the above results we may conclude that either variation of the order of merit method, the method of paired comparisons, or the graphic rating method may be employed to determine the affective value of colors and approximately the same results be obtained. The method best applicable to the experimental situation may be employed. It is probably best, however, to use one of the O.M. methods, or the method of P.C., since their reliabilities are considerably higher than that of the graphic rating procedure.

Part II. For musical intervals, the order from most to least preferred was determined by the method of paired comparisons and by the graphic rating method in the same manner as for the stimuli in Part I. The reliability coefficients, computed as in Part 1, follow:

Group 1 (16 *S*'s), paired comparisons; $r = .908$, $r_{\infty} = .952$.
Group 1 (16 *S*'s), graphic rating; $r = .936$, $r_{\infty} = .966$.
Group 2 (20 *S*'s), paired comparisons; $r = .814$; $r_{\infty} = .897$.
Group 2 (20 *S*'s), graphic rating; $r = .908$, $r_{\infty} = .952$.

The above coefficients reveal high reliabilities for both the method of paired comparisons and the graphic rating method in rating musical intervals for affective value.

When the results for the method of paired comparisons were correlated with those for the graphic rating scale the following coefficients resulted:

Group 1 (16 *S*'s), P.C. and G.R.; $r = .917$.
Corrected for attenuation, $r = .956 \pm .006$.
Group 2 (20 *S*'s), P.C. and G.R.; $r = .861$.
Corrected for attenuation, $r = .932 \pm .029$.

The raw intercorrelation coefficients, .917 and .861, indicate a marked relationship between the results from the two methods. When the coefficients are corrected for attenuation, they become .956 and .932, respectively. These coefficients demonstrate that, while the two methods are not exactly equivalent in this situation, the results in one method closely approximate the results in the other. For practical purposes one method may be considered equivalent to the other. Hence, the method best suited to the individual experimental situation should determine which is to be used.

In general, the graphic rating scale has been used to obtain quantitative estimates of abilities and personality traits (2, 4). The re-

sults of our investigation indicates that it may be employed to good advantage for rating the affective value of colors and of musical intervals. More investigations are needed to ascertain whether this procedure can be validly applied to other problems where stimuli are to be ranked according to some trait or according to preference.

When a psychological experiment is planned for a class in the drill laboratory or for investigating a research problem, choice of method is important. It has been demonstrated above that in certain experimental situations there is a choice of from two to four equivalent methods. If two methods do not yield equivalent results, the method which is most valid should be employed. If two or more methods are equivalent, however, choice of the one most suitable for the experimental situation should be made. Consideration of the following points should facilitate the choice: (*a*) Purpose of the experiment. One may wish to compare results obtained from two different methods, or one may desire to introduce a new method at this point in the laboratory course. (*b*) Reliability of the method to be used. Other things being equal, the most reliable method should be employed. (*c*) Convenience in manipulating stimuli and computing results. Some methods involve much more labor than others both in manipulation of stimulus material and in computing results, i.e., paired comparisons is more complicated than the order of merit method in these respects. (*d*) Tediousness to subjects. The graphic rating scale and the order of merit method are usually more interesting to subjects than the method of paired comparisons. (*e*) Time consumed in gathering data. The order of merit method and the graphic rating methods are both shorter than the method of paired comparisons when used individually or for group testing.

Summary and Conclusions

1. An analysis of the experimental literature reveals that the order of merit method is approximately equivalent to the method of paired comparisons in many experimental situations, but not in all.

2. In this investigation the order of merit method, the method of paired comparisons, and the graphic rating method have been compared with each other in two experimental situations: (*a*) for determining the affective value of colors, and (*b*) for determining the affective value of musical intervals (by last two methods only).

3. In these experimental situations the three methods yielded results which are approximately equivalent to one another.

4. The graphic rating technique has only a moderately high reliability in estimating the affective value of colors, but a very high reliability in judging the affective value of musical intervals. The reliabilities of the other methods were high in both experimental situations.

5. In several experiments in the psychological laboratory and in experimental investigations, where stimuli are to be ranked for relative excellence or value, there is a choice of two or more equivalent methods. In such cases the method used should be the one best adapted to the experimental situation. In choosing one of two or more equivalent methods the following factors should be considered: (*a*) purpose of the experiment; (*b*) reliability of the method to be used; (*c*) convenience in manipulating stimuli and computing results; (*d*) tediousness to subjects; and (*e*) time consumed in gathering data.

References

1. Barrett, M. A comparison of the order of merit method and the method of paired comparisons. *Psychol. Rev.*, 1914, **21**, 278-294.
2. Bingham, W. V. D., & Freyd, M. Procedures in employment psychology. Chicago: A. W. Shaw, 1926. Pp. 280
3. Brentlinger, W. H. A study of judgment by the method of paired comparisons, versus the order of merit method. (M. A. thesis.) Ohio State Univ., 1925.
4. Burtt, H. E. Principles of employment psychology. Boston: Houghton Mifflin, 1926. Pp. 579.
5. Cattell, J. M. The time of perception as a measure of differences in intensity. *Phil. Stud.*, 1902, **19**, 63-68.
6. DeCamp, J. E. The influence of color on apparent weight. *J. Exper. Psychol.*, 1917, **2**, 347-370.
7. Foster, W. S. Experiments in psychology. New York: Holt, 1923. Pp. x+309.
8. Foster, W. S., & Tinker, M. A. Experiments in psychology. (Rev. ed.) New York: Holt, 1929. Pp. xvi+392.
9. Gordon, K. Esthetics of simple color arrangements. *Psychol. Rev.*, 1912, **19**, 352-363.
10. Hevner, K. A comparative study of three psychophysical methods. *J. Gen. Psychol.*, 1930, **4**, 191-212.
11. Strong, E. K., Jr. The relative merit of advertisements. *Columbia Univ. Contrib. Phil. & Psychol.*, 1911, **19**, No. 2
12. Taylor, C. D. Visual perception versus visual plus kinaesthetic perception in judging colored weights. *J. Gen. Psychol.*, 1930, **4**, 229-246.

University of Minnesota
Minneapolis, Minnesota

LES MÉTHODES EMPLOYÉES POUR METTRE LES STIMULI EN ORDRE DE RANG

(Résumé)

Dans cette investigation le méthode d'ordre de mérite, celle de comparaisons mises en paires, et celle d'évaluation graphique ont été comparées l'une avec l'autre dans la détermination de la valeur affective des couleurs, et les deux dernières méthodes ont été comparées dans la détermination de la valeur affective des intervalles musicaux. Dans ces situations expérimentales les trois méthodes ont donné des résultats approximativement équivalents. La technique de l'evaluation graphique a une assez bonne constance quand on l'emploie pour estimer la valeur affective des couleurs, et une très bonne constance dans le jugement de la valeur affective des intervalles musicaux. Les constances des autres méthodes ont été bonnes dans les deux situations expérimentales.

TINKER

METHODEN UM REIZE ZU SCHÄTZEN

(Referat)

In dieser Untersuchung wurden die Methode der Wertordnung (order of merit method), die Methode gepaarter Vergleichung (method of paired comparisons), und die Methode graphischer Schätzung (graphic rating method) miteinander verglichen, und zwar durch die Bestimmung des affektiven Wertes von Blumen. Die letztern beiden Methoden wurden ferner miteinander durch die Bestimmung des affektiven Wertes von musikalischen Intervallen verglichen. In diesen drei experimentellen Situationen ergaben die drei Methoden Ergebnisse, die ungefähr miteinander übereinstimmen. Die Technik graphischer Schätzung weist eine mässig hohe Zuverlässigkeit auf, wenn sie zur Schätzung des affektiven Wertes von Farben gebraucht wird, und eine sehr hohe Zuverlässigkeit in der Schätzung des affektiven Wertes musikalischer Intervalle. Die Zuverlässigkeit der anderen Methoden war hoch in beiden experimentellen Situationen.

TINKER

SOME EMPIRICAL TESTS OF THE METHOD OF PAIRED COMPARISONS*

From the University of Nebraska

J. P. GUILFORD

In a previous article (4) the writer discussed the method of paired comparisons as a psychometric method, giving two short-cuts for finding scale-values of stimuli in order to reduce the labor, both in the experimental procedure and in the computation of the results. One of the methods was theoretically justified by basing it upon the method of constant stimuli. At that time no data were at hand with which to determine the relative validity of the three methods, Thurstone's original method[1] and my two short-cuts to test the assumptions which were made, or to answer the many questions which arose.

Since that time, the writer has secured some data which will serve this purpose. These data come from experiments with lifted weights, so we have objective stimulus-values with which to compare the calculated scale values. In this paper the writer hopes to bring out in more detail the relationship that exists between the constant method and the method of paired comparisons, and to make empirical tests of the methods for determining scale-values from comparative judgments. The method of paired comparisons promises to take us very far in the accurate measurement of psychological values in general, affective and appreciative, as well as sensory and perceptual. These empirical tests of the method seem, therefore, to be very much worth while. Incidentally, the data tell us something about the number of stimuli which need to be used as standards for comparison. They also have some bearing upon Fechner's law and its relation to the method of constant stimuli.

In these experiments, seven weights were used as stimuli: 185, 190, 200, 205, 210, and 215 grams. We will refer to these stimuli as R_1, R_2, R_3, R_4, R_5, R_6, R_7, respectively. R_{147} will mean the three stimuli, 185, 205, and 215 grams, respectively. Two indi-

*Accepted for publication by Carl Murchison of the Editorial Board and received in the Editorial Office, March 25, 1930.

[1]Thurstone's procedure is best seen in use in his article (7).

viduals, Mr. John Sanders (*S*) and the writer (*G*), alternated as experimenter and subject. Both subjects compared each weight with every other one in the series a hundred times. Each weight was used in turn as a standard, as in the method of constant stimulus differences, for 35 judgments in succession. From 140 to 210 comparative judgments were obtained in a single hour's observations. As a result, we have two sets of data, one for each subject, giving the proportion of judgments of "heavier" for each weight as compared with every other. No "equal" judgments were permitted. Each set of data contains 49 proportions, seven for each standard. Table 1 gives one set of these data for subject *S*.

TABLE 1

PROPORTIONS OF JUDGMENTS "HEAVIER" FOR EACH WEIGHT AS COMPARED WITH EACH STANDARD, FROM SUBJECT S, WITH *G*-VALUES COMPUTED

The standards	The variable stimuli						
	185	190	195	200	205	210	215
185	.48	.65	.78	.92	.93	.95	.99
190	.35	.50	.69	.80	.85	.97	.94
195	.22	.31	.48	.63	.72	.89	.91
200	.08	.20	.37	.52	.67	.78	.86
205	.07	.15	.28	.33	.54	.64	.74
210	.05	.03	.11	.22	.36	.46	.62
215	.01	.06	.09	.14	.26	.38	.56
Means of proportions	.180	.273	.400	.509	.619	.724	.803
Means minus .500	—.320	—.227	—.100	.009	.119	.224	.303
G-values	—.915	—.604	—.253	.023	.303	.595	.852

According to Thurstone's procedure (7), one next subtracts .50 from each of these 49 proportions, and then by reference to Pearson's Table I, or to Holzinger's Table XII (5), one finds the distance of each weight from the standard in sigma-units. These scale-values are given in Table 2 for the proportions in Table 1. Lastly, one finds the arithmetic mean of the sigma-values for each weight, and this is taken to be the final scale-value for that weight. These scale-values will be referred to as the *T*-values hereafter, and the series of values will compose what we will call the *T*-scale.

The writer suggested a shorter method in which we find the arithmetic mean of the proportions for each stimulus *before* referring to the tables to find the sigma-value. This method will be found

TABLE 2

THE *T*-VALUES FOR EACH OF THE PROPORTIONS GIVEN IN TABLE 1, WITH THE FINAL *T*-VALUES COMPUTED

The standards	The variable stimuli						
	185	190	195	200	205	210	215
185	— .050	.385	.772	1.405	1.476	1.645	2.326
190	— .385	.000	.496	.842	1.036	1.881	1.555
195	— .772	— .496	— .050	.332	.583	1.227	1.341
200	—1.405	— .842	— .332	.050	.440	.772	1.080
205	—1.476	—1.036	— .583	— .440	.100	.359	.643
210	—1.645	—1.881	—1.227	— .772	— .359	— .100	.306
215	—2.326	—1.555	—1.341	—1.080	— .643	— .306	.151
Final (mean) *T*-values	—1.151	— .775	— .324	.048	.376	.782	1.057

worked out at the bottom of Table 1. Incidentally, it was found that the means of the proportions themselves correlated almost perfectly with the *T*-values (4, p. 497) and hence might be used as scale-values if one were indifferent as to the unit of his scale. We shall test this finding again with these data, but we are more concerned with the *G*-method as outlined in the first part of this paragraph. The *G*-method has certain technical advantages. Among them is the fact that, in order to find the average proportion for any stimulus, one need know only the total number of choices received by that stimulus and not the percentage of choices between every pair of stimuli.

It may be convenient to repeat here the formula by which this is done (4, p. 499):

$$Av.\ Pc. = \frac{N_k + N_s/2}{N_s N_r}$$

in which N_k is the total number of choices given to a certain stimulus R_k; N_s is the number of subjects, or, as in this experiment, it is the number of comparisons of each pair; and N_r is the number of stimuli. In this experiment, however, each stimulus was compared with itself, hence the formula becomes simply, $Av.\ Pc. = N_k / N_s N_r$.

Another advantage of the *G*-method is that we need to look up in the table only N_r sigma-values instead of $N_r(N_r—1)/2$, or even $(N_r)^2/2$ sigma-values, N_r being the number of stimuli. The scale which is derived from this method we shall refer to as the *G*-scale, and the scale-values as *G*-values.

Several problems are now set for us. First of all, which values,

the *T*-values or the *G*-values, correlate better with the stimulus-values (*R*-values)? According to theory, the *T*-values lie on a psychological continuum, and so they should be more properly correlated with the log *R*-values. As a matter of fact, they do correlate better with the log *R*-values (see Table 1). But the *G*-values also correlate better with the log *R*-values than they do with the *R*-values. This indicates that the *G*-values also lie on the psychological continuum, contrary to the writer's earlier conclusion (4, p. 504). This conclusion was a natural one. We have always assumed, in the *phi-gamma* hypothesis, that the observed proportions have an ogival relationship with the *R*-values. It is the first assumption of the *G*-method that the scale-values can be determined from the observed proportions via the *phi-gamma* function, and hence the scale-values must, accordingly, lie upon a physical or objective continuum. The writer deliberately rejected the possibility that the ogival relationship holds rather between the proportions and the log *R*-values. Since then, Thurstone has demonstrated this very point and has discussed it at length (8).

Let us look at Table 3 for the answer to our first problem. The coefficients in that table are the product-moment coefficients of correlation. They are extremely high in all cases and the differences between them are very small. On the one hand, this indicates reliable scale-values, but, on the other hand, it makes comparison of our different methods less unambiguous than one might wish. Still, what differences do exist are rather uniform throughout, and it must be remembered that the probable errors of such high coefficients are very small, even with only seven cases. They range from .010 for a coefficient of .980, through .002 for one of .995 to .0005 for one of .999.

From Table 3 one would conclude that the *T*-values and the *G*-values are about equally reliable, whether correlated with the *R*-values or with the log *R*-values. Both the *T*-values and the *G*-values correlate higher with the log *R*-values than with the *R*-values. The differences are very small in view of the fact that the relationship between *R* and log *R* themselves for this short range of *R*-values is almost linear. This result holds for the single standards and is accordingly a strong verification of Fechner's law. It makes us question whether in the method of constant stimuli we should not space our stimuli according to log *R* instead of *R* as in the present

TABLE 3

Product-moment coefficients of correlation between the *T*-values and *G*-values and the *R*-values and log *R*-values, when the scale-values are derived from seven, five, three, and one standard stimuli.

Correlation with stimuli in the standard	Subject S				Subject G			
	R-values		log *R*-values		*R*-values		log *R*-values	
	Values		Values		Values		Values	
	T-	*G*-	*T*-	*G*-	*T*-	*G*-	*T*-	*G*-
1234567	.998	.998	.999	.999	.980	.991	.992	.992
23456	.994	.997	.999	.998	.989	.991	.990	.990
147	.994	.993	.995	.997	.996	.990	.997	.992
246	.996	.992	.998	.993	.989	.996	.990	.996
345	.996	.995	.996	.996	.998	.990	.990	.990
Mean of last three coefficients	*.995*	*.993*	*.996*	*.995*	*.994*	*.992*	*.992*	*.993*
1	.982		.985		.973		.975	
2	.969		.970		.976		.974	
3	.990		.995		.981		.987	
4	.997		.997		.986		.987	
5	.993		.994		.928		.931	
6	.977		.974		.974		.984	
7	.989		.991		.981		.984	
Mean of last seven coefficients	*.985*		*.987*		*.971*		*.975*	

procedure. Probably no serious error is involved in the method as it is now conducted due to the close linear correlation between *R* and log *R* over the short range of stimuli usually employed, but it might be wise to follow Galton's procedure and make the stimulus-series a geometric one (2, pp. 34 ff., 379 ff.).

A second problem is concerned with the number of standard stimuli. The writer suggested in the earlier article that the number might be less than the number of stimuli, for if the number of stimuli is large the task of paired comparisons becomes burdensome. In this experiment we have tested the reliability of the scale-values with seven, five, three, and one stimuli. A subsidiary problem has to do with the selection of stimuli for standards when the number is limited. Should one scatter them over the whole range or limit them to a group near the middle of the range? And, taking only one standard, should it be selected near the middle of the range? A priori, one would expect a medium standard to give better results, since it would

yield a minimum number of proportions of .00 and 1.00, proportions which are highly disturbing to the T-method especially. In order to answer these questions, the T-values and G-values have been computed for R_{23456}, R_{345}, R_{246}, and R_{147}, and for each stimulus alone, as standards. The scale-values in each case have been correlated with the R-values and the log R-values. The results will be found in Table 3.

As we reduce the number of standards from seven to one, the reliability of the scale-values diminishes noticeably, but not significantly, until only one standard remains. If three standards are used, it seems at first sight to make little difference whether they are grouped near the middle of the range or scattered as widely over it as possible. It does make a difference, however, if we consider only the correlation of the scale-values with log R. In this case, the scattered standards give superior scale-values. Since correlation with the log R-values constitutes the real test of the scale-values, we may grant some degree of superiority to the scattered standards. If single standards are used, the middle one in the series is the best in every case, but the remaining ones come in haphazard order, so that no general rule covering all cases can be stated. It will take a greater range of stimuli to answer these questions more unequivocally.

Incidentally, the averages of the observed proportions correlate better with either the R-values or the log R-values than do the T-values or G-values, and, as the writer has previously pointed out, the average proportions correlate almost perfectly with the T-values. Table 4 gives the results. In this experiment it would seem that the average proportions give better scale-values than either the T-method or the G-method. This very high agreement is due in part to the linear nature of the psychometric function in the limited range from about $-1.00\ \sigma$ to $+\ 1.00\ \sigma$. It is not entirely due to that fact, for the means of the proportions give a linear plot against the T-values over a wider range, from $-1.50\ \sigma$ to $+1.50\ \sigma$ if the data

TABLE 4

COEFFICIENTS OF CORRELATION BETWEEN THE MEANS OF THE PROPORTIONS AND THE VARIOUS VALUES

Subject	R-values	log R-values	T-values
S	.999	.999	1.000
G	.994	.995	.999

are extended, as we have done in the following discussion (see Figure 3).

It is very desirable that our data be extended for several reasons which have already been mentioned. No more observations have been taken, but the next best thing has been done. The raw data have been fitted to the best-fitting ogives and the theoretical proportions of each ogive have been extended from .00 to 1.00. Figure 1 shows the seven ogives for the better subject. The actual proportions are shown plotted in solid lines and the theoretical ones in dotted lines. Vertical lines have been constructed to mark the ends of the observed data. The very heavy ogive is drawn through the means of the proportions. This procedure gives us a range from about 155 gr. to about 245 gr., in terms of *R*-values, for which we can now determine scale-values by both the *T*-method and the *G*-method.

We are now prepared to answer several questions, provided the reader is willing to accept what has been done so far. We might just as well have taken a series of theoretical ogives of identical slope, but the ones we have used are at least partly empirical and will probably give us more clear-cut answers because they are partially freed from errors of observation.

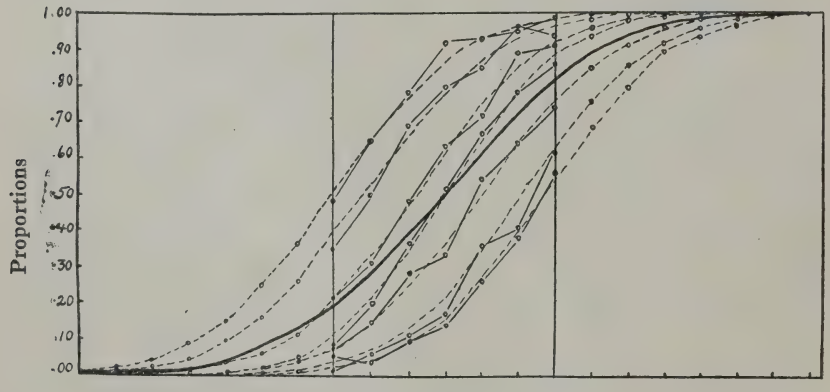

FIGURE 1

SHOWING THE SEVEN OGIVES AND THEIR ARITHMETIC MEAN, WHICH IS ALSO AN OGIVE

Between the two verticals the actually observed data are plotted in solid lines. The dotted lines represent the best-fitting ogives for these data.

First of all, this procedure tests an important assumption of the *G*-method, the assumption that the means of the proportions obey the *phi-gamma* hypothesis; that they have an ogival relationship with the log *R*-values just as the proportions from which they are derived. Each of these curves has been plotted with the abscissa units equal to *K* log *R*, *K* being undetermined. What the reader should notice is that the average proportions form a very good ogive of a lower precision than the original functions. This bears out the earlier result, that the *G*-values correlate better with log *R* than with *R*.

No doubt there is mathematical proof for this fact, but the experimental data alone prove the point in still another way. The writer has tested the goodness of fit of the data to their ogives when one, three, five, and seven standards were used and an average ogive computed. The Pearson *P*-coefficient for goodness of fit increases directly with the number of standards. In the case of subject S the *P*'s are .938, .995, .996, and .999, respectively; for subject G they are .835, .877, .925, and .958. All of these coefficients indicate superlatively good fits, according to accepted standards (1). In these comparisons, however, a constant number of observations, namely 100, has been assumed for the value of *k*. If we admit that *k* increases to 300, 500, and 700 as the number of standards increases to three, five, and seven, the results are about the same for subject S, but not for G. The *P*-coefficients in this case are, for S, .938, .890, .879, and .950; for G, .855, .237, .091, and .048. It is difficult to assign a just value for *k* in these comparisons. When we increase the number of standards to seven, it is true that each stimulus is compared 700 times, but not with the same standard. In spite of this unfair comparison, in the case of subject S at least, the averages of the proportions give very good ogives.

The extended ogives answer another question for us. Within the limited range of the seven stimuli it was found that the *T*-values and the *G*-values correlated equally well with log *R*. Will this be true if the range is extended? Apparently not, if Figure 2 is to be relied upon. It shows an excellent linear relationship between the *G*-values and log *R* throughout a range almost three times that of the original seven stimuli, whereas the *T*-values are linear for a very short distance beyond the original range. Taking in the total range, the *T*-values seem to have an ogival relationship with the *G*-values. This fact was anticipated by the writer in the previous

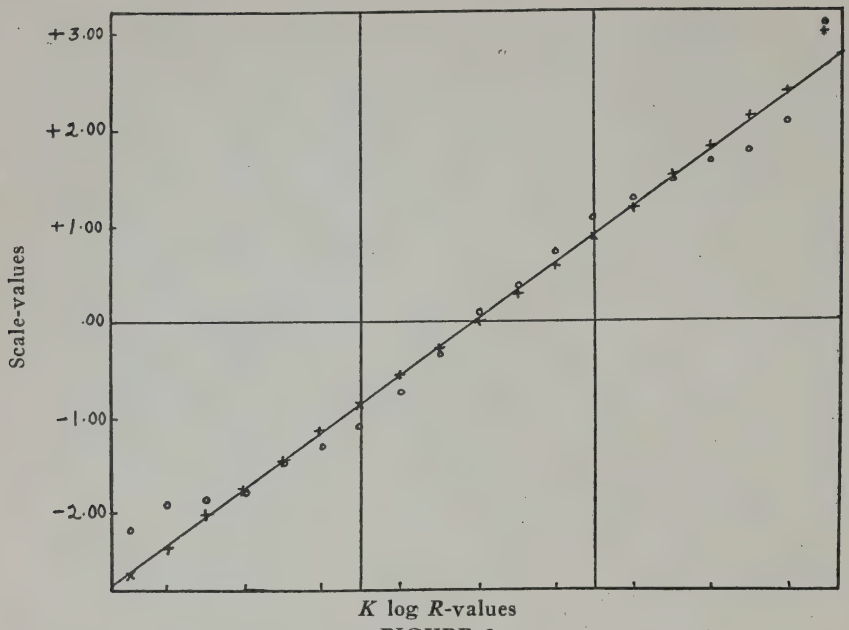

FIGURE 2

SHOWING THE RELATIONSHIP BETWEEN K LOG R AND THE G-VALUES, ON THE ONE HAND, AND THE T-VALUES, ON THE OTHER

The G-values are represented by crosses, the T-values by circles.

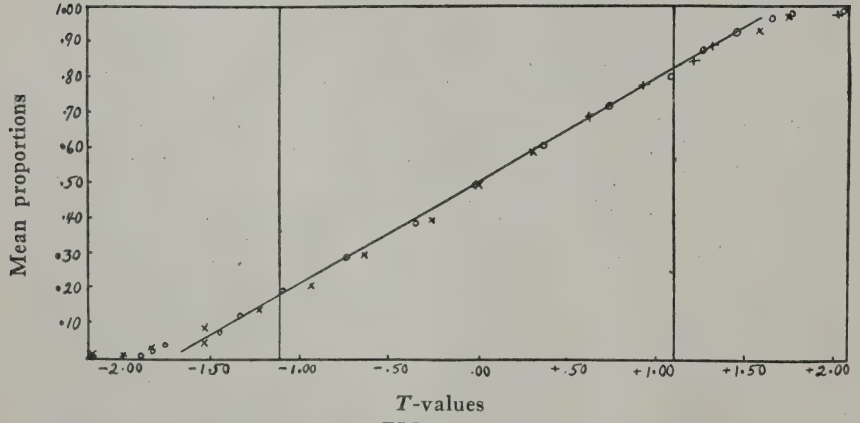

FIGURE 3

SHOWING THE RELATIONSHIP BETWEEN THE AVERAGE PROPORTIONS AND THE T-VALUES FOR BOTH SUBJECTS

The circles are the scale-values for Subject S and the crosses for Subject G.

article, and it was suggested by the recurrent fact that the T-values correlate so well with the means of the proportions. But even this linear relationship does not hold far beyond the seven original stimuli, as will be seen in Figure 3.

It will be noticed in Figure 2 that the points near the two ends of the scale deviate very decidedly from the curves. This is especially true of the T-values. It is due to the fact that so many of the proportions at the two extremes are .00 or 1.00. In either case, the corresponding T-values are indeterminate, and the final or mean T-scale values for these stimuli are based upon only one or two proportions. This difficulty seems to be overcome, to some extent, in the G-method, for the average proportions may be obtained from all seven standards with reliable scale-values even when all but two of the seven stimuli are .00 or 1.00. When more than two of the proportions are .00 or 1.00 in the T-method, the scale-values deviate seriously from the true values. By "true" values is meant those values which correlate perfectly with log R.

While the G-method seems better able to take care of the observed proportions of .00 and 1.00, and hence competent to give reliable extreme scale-values, a troublesome problem still remains. It is the problem of the unit. The T-method has a unit which is based upon good logical theory (9), and it is independent of the number of stimuli which may be regarded as standards. The unit is equal to $\sqrt{2}\sigma_a$, in which σ_a is the standard deviation of the discriminal dispersion of the standard stimuli, the dispersions of which are assumed to be equal. This unit is true for the G-scale values only when the standard is a single stimulus, for then, of course, the T-method and G-method are identical in procedure.

As the number of standard stimuli increases, the unit becomes larger; the precision of the average ogive becomes less. For example, compare the slope of the average ogive in Figure 1 with the slope of one of the other ogives from which it is derived. If the number of standards were increased without limit, the precision of the average ogive would approach zero and the unit would become infinite. As the number of standards was increased in these experiments from one to three, five, and seven, the corresponding measures of precision, h, were as follows: For subject S, they were .054, .051, .046, .041, respectively; for subject G, they were .048, .045, .044, and .037, respectively.

It might seem that this difficulty could be obviated in one of two ways. Either we would have to agree upon a uniform number of standard stimuli in practice, or else we would have to find a way to correct the unit and to reduce it to the one which holds for a single standard. It is very probable, however, that the size of the unit is not only a function of the number of standards, but also of their spacing. For example, if we included but three stimuli in our composite standard, would the change in the unit be affected by the extent to which these three stimuli scatter over a wider or narrower range? The measures of precision, h, have been computed for our three cases, R_{345}, R_{246}, and R_{147}. They are almost identical with the h's for three, five, and seven standards, respectively. For subject S they are .051, .044, and .041, respectively. For subject G they are .045, .043, and .037, respectively. Compare these constants with those in the last paragraph. They are almost identical.

Undoubtedly the distance between the standards is the chief determining factor in the size of the unit in the G-method. This almost closes the door to either of the remedies suggested above. Even if some adjustment could be made in the scale-values, which is based upon the divergence of the standard stimuli, in most cases we could not depend upon the R-values to determine that divergence beforehand. We would have to depend upon the divergence between the G-values of the standards when there are no R-values. In many problems, however, the size of the unit is of little consequence; it is the relative scale-values that we want. When this is the case, the G-method is not handicapped by the problem of the unit.

What conclusions and recommendations for future practice can be made from the preceding discussion?

1. The G-method has several advantages over the T-method, namely,

a. It reduces considerably the work involved in the collection of data and in the computation of scale-values.

b. It seems to give more valid scale-values, especially over a wider range of stimuli, when proportions of .00 and 1.00 occur. The test of validity is the product-moment coefficient of correlation with the log R-values.

2. In both methods, the number of standards can be reduced to a relatively small number, three or five, in the case of lifted weights,

without serious reduction of the accuracy of the results. Two qualifications must be added here:

a. If the number of standards is reduced, it is probably best to select those which scatter widely and evenly over the whole series.

b. This will probably depend upon the kind of stimuli with which one is dealing.

3. The *T*-method has the advantage of a theoretically defined unit which is independent of the number of standards and their spacing. This defect of the *G*-method may be cared for

a. by correcting the unit for alterations due to the number and spacing of standards; or

b. by the fact that the unit is often an inconsequential factor. Whenever the unit is of consequence, one can limit the number of standards to a convenient number and use the *T*-method.

As a final suggestion, the writer proposes that the unit of the psychological scale be the probable error of the ogive rather than the standard deviation. This makes the unit the old familiar and more significant "just noticeable difference" of psychophysics. We then have a unit which is a difference that has a probability of occurrence of .75.[2] Thorndike suggested years ago that the probable error be used as a scale-unit and gives tables for transmuting proportions into scale-differences (6).

Summary

A comparative study of Thurstone's method of deriving scale-values from data obtained by the method of paired comparisons and that of the writer was made by using data which was obtained from lifted weights. The reliability of the methods is measured by correlating the scale-values with the logarithms of the stimuli.

Certain assumptions made about the *G*-method were tested empirically and found to be valid. The effect of reducing the number of standard stimuli was tested for both methods.

In most respects the *G*-method proved to be the more convenient and valid method, but it has one weakness in that the unit is a variable factor. Suggestions are made for future practice in the scaling of psychological values.

---

[2]Scale-values based upon the j.n.d. as the unit may be found by using Table XI in Garrett (3).

References

1. Culler, E. The normal law; its place in quantitative psychology. *J. Exper. Psychol.*, 1926, **9**, 271-298.
2. Galton, F. Inquiries into human faculty. London, 1883.
3. Garrett, H. E. Statistics in psychology and education. New York: Longmans, Green, 1926. Pp. 330.
4. Guilford, J. P. The method of paired comparisons as a psychometric method. *Psychol. Rev.*, 1928, **35**, 494-506.
5. Holzinger, K. J. Statistical tables for students in psychology and education. Chicago: 1925. Pp. v+74.
6. Thorndike, E. L. Introduction to the theory of mental and social measurements. (2nd ed.) New York: Teachers Coll., 1913. Pp. xi+277.
7. Thurstone, L. L. The method of paired comparisons for social values. *J. Abn. & Soc. Psychol.*, 1927, **21**, 384-400.
8. ———. The phi-gamma hypothesis. *J. Exper. Psychol.*, 1928, **11**, 293-305.
9. ———. Psychophysical analysis. *Amer. J. Psychol.*, 1927, **38**, 368-389.

University of Nebraska
Lincoln, Nebraska

QUELQUES TESTS EMPIRIQUES DE LA MÉTHODE DES COMPARAISONS MISES EN PAIRES

(Résumé)

On a fait une étude comparative de la méthode Thurstone de dériver les valeurs d'échelle des données obtenues par la méthode des comparaisons mises en paires et de celle de l'auteur, en employant des donneés obtenues des poids levés. On mesure la constance de la methode au moyen de corréler les valeurs d'échelle avec les logarithmes des stimuli.

On a testé empiriquement certaines suppositions faites sur la méthode G et les a trouvées valables. On a testé pour les deux méthodes l'effet de réduire de nombre des stimuli étalons.

En général la méthode G s'est montrée plus commode et plus valable, mais elle a un inconvénient puisque l'unité est un facteur variable. On offre des suggestions pour la pratique future dans la mise en échelle des valeurs psychologiques.

Guilford

EINIGE EMPIRISCHE TESTS ANDER METHODE GEPAARTER VERGLEICHUNG

(Referat)

Es handelt sich um eine vergleichende Untersuchung der Thurstone'-schen Methode zur Gewinnung von Skalenwerten von Angaben, die durch die Methode der gepaarten Vergleichung erhalten wurden, und der Methode des Authoren, mittels Angaben, die aus gehobenen Gewichten erzielt wurden. Die Zuverlässigkeit der Methode wurde durch Korrelieren der Skalenwerte mit den Logarithmen der Reize gemessen.

Es wurden gewisse Annahmen über die G-Methode empirisch geprüft und gültig befunden. Es wurde ferner der Effekt der reduzierten Anzahl von Normalreizen für beide Methoden geprüft.

In meister Hinsicht erwies sich die *G*-Methode als die bequemere und gültigere, aber sie hat eine Schwäche, indem ihre Einheit ein variabler Faktor ist. Es werden Vorschläge gemacht für die zukünftige Prüfungstechnik psychologischer Werte.

GUILFORD

SHORT ARTICLES AND NOTES

EDITED BY
F. C. Bartlett
V. Borovski
F. Kiesow
Matataro Matsumoto
Walter Miles
R. W. Wilcocks

A NOTE ON PERIPHERAL FORM DISCRIMINATION[1]

Norman L. Munn and George A. Geil

I. Introduction

The senior author became interested in the present problem while he was unsuccessfully attempting to obtain visual pattern discrimination in rats and raccoons. Until the recent work by Lashley (6) and Munn (7) experimenters had been unable unequivocally to demonstrate the existence of pattern vision in infra-primate mammals. Since none of this group of animals possesses a foveal depression or well-differentiated sensitive area, it was suggested by a number of investigators that a foveal depression might be an essential prerequisite for pattern or form vision. One method of determining the validity of this assumption would be to determine whether human subjects, by using peripheral vision alone, could discriminate between visual forms such as those presented in the various experiments with animals. If it were found that the human subject could discriminate form outside of direct foveal vision it would, by analogy, seem logical that animals without a foveal depression might likewise discriminate if the experimental conditions were conducive to such discriminations.

A number of experimenters have already worked in this field with human subjects (1,3,4,5,8,9) but the conditions of experimentation have not been as well-controlled as in the animal experiments. In animal discrimination experiments the stimuli are of equal area and brightness; the animals are required to make a differential response to two stimuli of different form or pattern; the stimuli are changed in position in a chance order; the

[1]This problem was suggested and carried out under the direction of the senior author. The apparatus is a result of the cooperative efforts of Glenn W. Geil and Lloyd Jones of the Physics Department of the University of Pittsburg, Carroll A. Whitmer of the Department of Psychology of the University of Pittsburgh, and the authors.

stimuli are stationary while the animal is discriminating; the movements of the experimenter are hidden from view; and the animals may use both eyes. In the typical perimetry experiment, however, some or all of the following features are present: (*a*) The subject is required to fixate a central point for an extended period of time while the experimenter moves the stimulus toward, or away from, the foveal region. The subject is thus exposed to the stimuli for an uncontrolled period of time. Furthermore, the task of attempting to maintain a steady fixation for such an extended period as is usually required leads to fatigue, which may seriously influence the results. Aubert (1), Kleitman and Blier (5), Zigler, Cook, Miller, and Wemple (8), and Wertheim (9) used this method. (*b*) Since the presentation of the stimulus is monocular it is necessary to blindfold or close one eye. This situation is obviously unnatural. All of the experiments on peripheral form discrimination involve this feature. (*c*) The stimulus is usually a reflecting surface and, as it is moved through the various positions in the visual field, its brightness changes from point to point. In only one instrument, the Ferree-Rand perimeter (2), is the brightness factor kept constant. (*d*) The stimulus moves while the subject is discriminating, thus introducing an extraneous factor that cannot be readily controlled. As Ferree and Rand point out, "The steadiness of fixation is greatly influenced by the method of giving the stimulation. One of the serious objections to a moving stimulus is the difficulty of holding a steady fixation while the object to be observed is moving. The alternate procedure is the use of a stationary stimulus. That is, the stimulus is placed at the desired point on the perimeter arm and covered with a preëxposure card. The observer takes his fixation and at a given signal the stimulus is exposed and re-covered" (2 p. 64). It is possible, moreover, that movement may enhance the stimulating value of the stimulus. All of the experimenters in this field except Ferree, Rand, and Monroe (3) and Geissler (5) have used the "moving method" of presentation. Unless the forms are covered, as suggested by Ferree and Rand (2), the subject may be stimulated by them at times other than those during which he is fixating. When a stimulus is moved from the juxta-foveal region toward the periphery, and is not changed between successive exposures, there is another source of error. Knowing what the nature of the stimulus is, the subject may be able to "guess" as to its nature when it can no longer be discriminated clearly. (*e*) Instead of being required to *discriminate* between two stimuli the subject is required to report the presence of but one stimulus. The reporting of two stimuli, beyond the fact that it is more comparable to work on animal discrimination than is the reporting of one stimulus, offers several advantages for control of the experimental conditions. When the subject is called upon to report two stimuli, one on the right and one on the left, or one up and one down, when they are momentarily exposed at exactly the same time, there is a greater tendency for fixation to remain constant

during the brief exposure period than if only one stimulus were to be reported, i.e., the "set" of the subject is different. In order to report both stimuli he *must* fixate the central point. The use of two stimuli offers the possibility of changing the right and left, or up and down, positions of the forms from trial to trial. New combinations can be inserted from time to time. (*f*) The experimenter is usually within the subject's view and may influence his responses.

Our aim in beginning this research was to discover whether human subjects could discriminate between certain forms by means of peripheral vision when the conditions of experimentation were as well controlled as we could devise. If the human subject could discriminate with peripheral vision, what combination of forms could be most accurately discriminated? This was a practical problem. The combinations of forms most readily discriminated in peripheral vision would be the combinations best adapted to work in animal discrimination when the animals lack a foveal depression or comparable sensitive area.

Since none of the available perimeters are adapted to the presentation of two forms of equal area and brightness, and since they all possess some of the above-mentioned inadequacies, it was necessary for us to devise an apparatus for our problem.

II. Apparatus and Technique

The general features of the apparatus and technique used in this experiment were as follows: Two forms of equal area and brightness were offered for discrimination. These stimuli were momentarily presented after the subject had taken the position of fixation. The subject observed with both eyes. The illumination of the forms was transmitted rather than reflected, thus the brightness did not greatly change from point to point in the visual field. The forms were not moved during the fixation period and were not observable before or after this period. (The aperture which emitted the form-like area of light was concealed behind a uniform white surface and the form did not appear until the light behind it was connected.) The experimenter manipulated the apparatus while outside of the subject's field of vision. The experimenter was so situated that he could see whether the subject moved his eyes during the "fixation" period. The forms differed from trial to trial in a chance order. The apparatus is pictured in Figures 1 and 2. For those who may wish to duplicate it the following detailed specifications are given.

The apparatus is supported by two large tripods (securely clamped to the table) to which the hollow cylinder *a* (encasing the central pivot shaft) is firmly held in place by two clamps from each tripod. With the set-screw control *b*, the perimeter may be rotated in either direction and held firmly in any meridional plane. On the end of the central pivot shaft *a* is mounted a pointer which indicates directly on a protractor scale *c* the exact meridional position. The perimeter is equally balanced in all possible

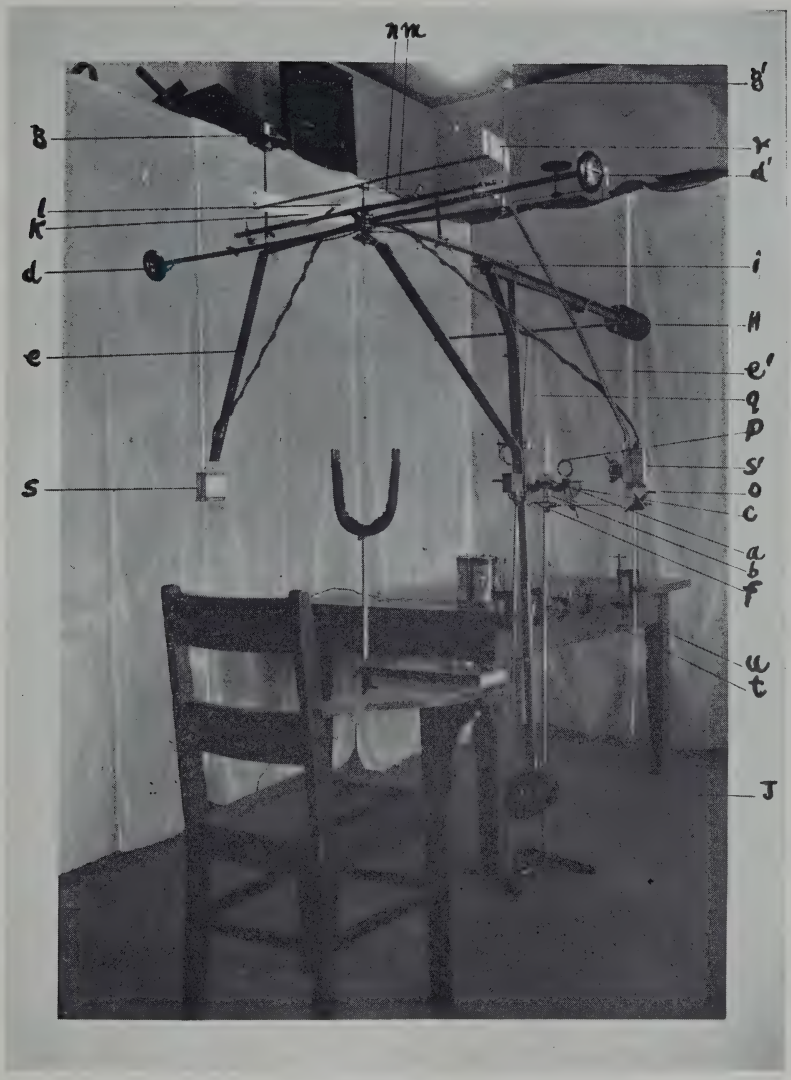

FIGURE 1

Apparatus Arranged for Testing on the Horizontal Meridian

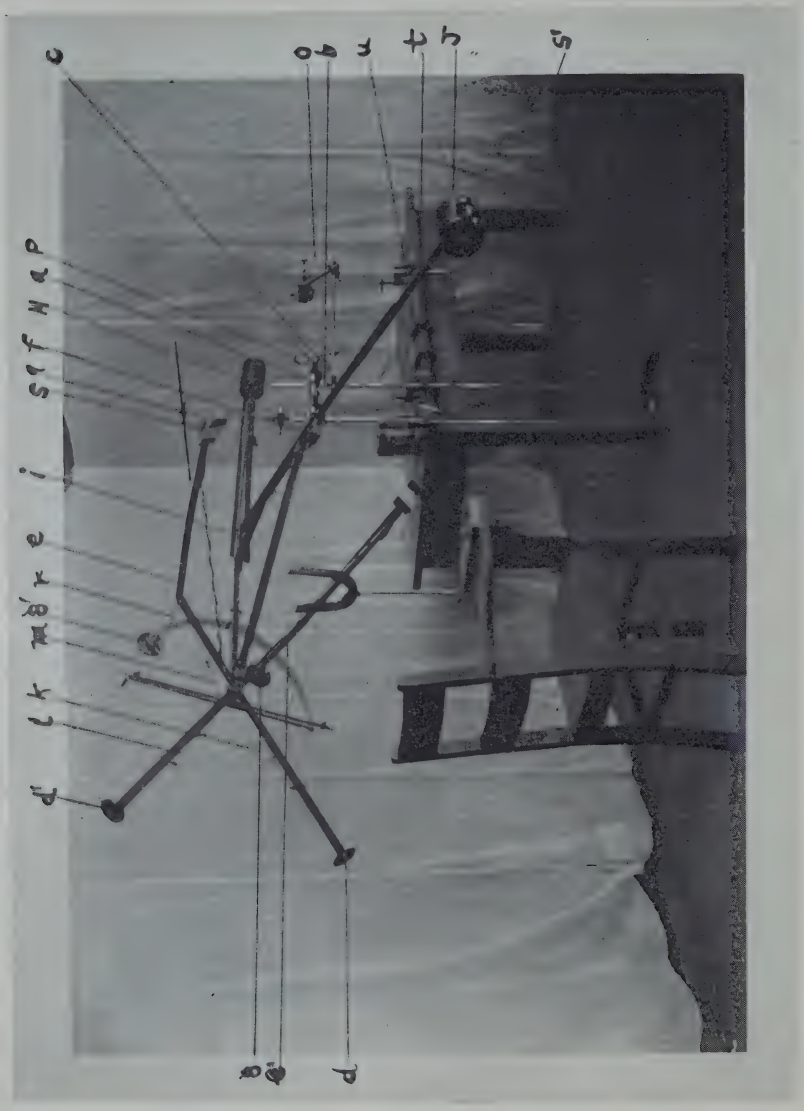

FIGURE 2
APPARATUS TILTED 45° TO THE LEFT

positions by means of a system of counterweights. The counterweights *d* and *d'* insure that the two arms *e* and *e'* are exactly balanced at all times regardless of the degree of rotation. This eliminates any tendency for either arm to precede or lag behind the other one when approaching or receding from the fixation point *f* in positions other than the horizontal. The counterweights *g* and *g'* correct for any twisting effect which might occur in the arms themselves when the perimeter is tilted from the horizontal position. Counterweight *h* is for the purpose of counterbalancing all the weight in front of the point *i*. Similarly, counterweight *j* exactly balances all the weight of the apparatus beyond the central rotational point. The wires *k, l, m,* and *n,* which are all under slight tension, effectively prevent any disturbing vibration of the arms when these are moved to new positions. By means of a special arrangement of pulleys these arms may be moved in or out as desired. The system of pulleys which controls the position of the stimuli in the field of vision is manipulated by means of the small crank *o*. The guide *p* insures even winding of the cord about the windless cylinder. This double arm control offers the possibility of presenting two forms simultaneously or one form at a time. (It is, of course, adapted to the use of other stimuli than form.) The movements of the experimenter were hidden by means of a cardboard screen suspended from the upright support *q*. (The screen was removed for the picture.) A pointer *r* is mounted on each arm. These pointers indicate on the degree scale mounted above the arms the position of the stimuli in relation to the fixation point *f* (a red cross). The two arms move reciprocally, hence the readings on each side of the fixation point are equal. The side of the scale from which one takes his reading, then, is purely a matter of convenience as determined by the rotational plane of the perimeter. The light boxes *s* and *s'* were made from highly polished thin sheet metal. Inside of each box was a 10-watt daylight bulb connected to a 110-volt A. C. circuit. The forms, all 10 sq. cm. in area, were carefully cut from the center of small squares of cardboard which fitted snugly into the grooves in front of the light boxes. The forms were covered by sheets of white paper. These were pasted over them so that they could not stimulate the subject until the light behind them was flashed on, i.e., the fronts of the light boxes were uniform white areas as pictured in Figures 1 and 2 until the lights behind them were turned on, they then comprised a very bright white form on a dimly illuminated white background. The lighting of the boxes was controlled by means of a knife switch *t* and two button switches. The circuit was so wired that, by closing the knife switch, the pressing of either button would simultaneously connect both stimuli. By opening the knife switch, either stimulus could be presented alone. The current for illuminating the fixation point was obtained from three dry cells. It could be turned off by opening a knife switch *u*. The general illumination of the room came from a 150-watt lamp covered by a frosted glass mantle. It was 6 feet above the fixation point.

The forms presented in the light boxes in various combinations determined by chance were a circle, a square, a diamond, a rectangle, a hexagon, and a triangle. Twenty-four different combinations of these forms were used. At times the forms in both light boxes were identical, at other times they differed.

The subjects were seated with their heads in a head-rest placed so that the distance from the forehead to the fixation point would be 60 cm. They

were instructed to fixate the red cross as soon as the experimenter gave the warning signal, "ready." They were told that two forms would be presented and that they were to attempt to report both of them. Approximately one second after the instruction to fixate had been given, the stimulus lights were momentarily flashed. This momentary presentation of the stimuli, the short period of fixation, and the "set" to report both forms effectively prevented a shift in the subject's eyes while the stimuli were exposed. (Observation of the eyes of a number of observers has shown that there is no shift in fixation during such an exposure of the stimuli.)

The forms were first presented in the periphery (85°) and were then moved toward the fixation point by steps of 5°. The subject attempted to make a report at each position of the stimuli. (Each form-combination had to be moved toward the fixation point a certain distance before the subject would venture to make a report.) After each exposure of the forms the subject's report was noted. The stimuli were moved in to the 5° position regardless of whether the subject was reporting them correctly. The subject was not advised concerning the accuracy of his reports.

To determine the limits of the field for each form on each of the quadrants (0, 45, 90, 135, 180, 225, 270, and 315 degrees) we averaged the points from which, as the forms were moved to the central position, the subject made no further errors. There was one point in each quadrant for each of the 24 form combinations. (The forms were never moved from the center of the field toward the periphery. We considered that moving them in from a point at which they could *not* be discriminated, would give much more reliable results than moving them in the other direction.) The combination was changed after each trial, a trial being the series of presentations on one quadrant from the 85° limit to within 5° of the center of the field. If we had presented the forms in the 5° limit and then moved them outward, the subject, since he could discriminate at the 5° limit, would now know their nature. Another, perhaps better, method of presenting the stimuli would be to change the combination after each presentation rather than after each trial. However, since each form was presented at each point in the visual field in a number of combinations, and since the general conditions of experimentation were controlled as they were, it was not necessary to repeat each combination of forms in each position a number of times.

Four subjects took part in the experiment. They were the senior author and three instructors from the Department of Psychology.

III. Results

The results demonstrate that all of the subjects were definitely able to discriminate forms by means of the peripheral retina, thus showing that a foveal depression is not an absolute prerequisite for the accurate discrimination of form. Although our method differed widely from that of pre-

vious investigators in this field, our general results were quite similar to theirs. In all of the experiments on peripheral form discrimination (1, 3, 4, 5, 8, 9) the subjects have been able to discriminate by means of the peripheral retina.

In our experiment, however, the shape of the forms was found to be quite important in determining the limits of peripheral discrimination. Although all of our forms were of the same area and brightness, the average distance from the fovea at which they could be discriminated depended upon the retinal distribution of their respective brightnesses. From Table 1 it will be seen that the hexagon had an average extension of 26°, while

TABLE 1

AVERAGE EXTENSION FOR EACH FORM IN DEGREES

Subject	Triangle	Circle	Diamond	Square	Rectangle	Hexagon
M	50	30	44	44	36	24
W	61	55	47	47	36	23
MH	57	45	46	37	44	36
H	52	48	47	38	24	21
Average	55	44.5	46	46.5	35	26

the triangle had an average extension of 55°. Next to the hexagon, the rectangle had the smallest field. The other forms did not show differences in extension as great as those between the triangle, hexagon, and rectangle. Isopteral charts were constructed for each subject. Inspection of these charts showed that, while the lines for the circle, square, rectangle, and diamond crossed and recrossed at various points, there was no crossing of the lines for the hexagon and triangle. None of the lines of the hexagon extended beyond the lines of the triangle at any point on the charts. Table 2, presenting the errors of report for each of the forms, demonstrates results similar to those of Table 1. The retinal distribution of light emitted by a form determined the correctness with which it could be reported. The triangle was reported erroneously only twice, whereas the hexagon was

TABLE 2

ERRORS OF REPORT FOR EACH FORM

Subject	Triangle	Circle	Diamond	Square	Rectangle	Hexagon
M	1	5	2	0	3	6
W	0	1	3	3	5	8
MH	0	3	5	5	5	8
H	1	3	4	7	7	8
Total	2	12	14	15	20	30

reported erroneously 30 times in the same number of presentations. The errors of report for the circle, diamond, square, and rectangle were, respectively, 12, 14, 15, and 20.

The comments which the subjects offered from time to time substantiated the fact that the hexagon was difficult to discriminate and that discrimination of the triangle was relatively easy. The large number of errors for the hexagon may be accounted for by the fact that it was often reported to be a circle until it was within a short distance of the fixation point.

Kleitman and Blier (5) found little difference in the ability to discriminate between a circle, square, triangle, and five-pointed star, but each of their forms was inscribed in a circle. Our forms were equal in area and brightness. Geissler (4), whose method was that of presenting a stationary form at various points in the periphery, found that of a square, a triangle, a diamond, a sector, and a circle, the circle was most easily and the sector least easily discriminated. The other experimenters in the field have used only one form or have been interested in other aspects of the problem.

Kleitman and Blier (5) discovered that the form field had a greater horizontal than vertical extension and that the temporal field was greater than the nasal field. Geissler (4) reports that he found fewer erroneous reports in the horizontal than in the vertical field.

The distribution of errors for the horizontal and vertical meridians in our experiment are presented in Table 3. It will be seen that, while the

TABLE 3

ERRORS OF REPORT ON THE VERTICAL AND HORIZONTAL MERIDIANS

Subjects	Vertical meridian		Horizontal meridian	
	Above	Below	Right	Left
M	2	1	2	2
W	3	2	4	2
MH	2	3	3	4
H	3	5	4	5
Total	10	11	13	13

difference between the two meridians is not great, the data do not support Geissler's data. Our results show the vertical meridian to be a little more accurately reported than the horizontal meridian. It should be remembered, however, that our method differed from Geissler's. Our subjects used both eyes while Geissler's subjects used one eye. In Table 4 are reported the extension of the vertical and horizontal fields for the triangle. (The other form fields are quite similar in shape.) It will be seen that the horizontal extension of the field for the triangle is much greater than

TABLE 4

EXTENSIONS OF THE HORIZONTAL AND VERTICAL FIELDS FOR THE TRIANGLE

Subjects	Vertical meridian		Horizontal meridian	
	Above	Below	Right	Left
M	35	40	55	55
W	40	45	70	75
MH	40	65	60	60
H	40	60	50	55
Average	38.7	52.5	58.7	61.2

the vertical extension. These results, then, agree with those of Kleitman and Blier (5). The upper vertical meridian is much smaller than the lower vertical meridian. This is due to the fact that the field of vision is restricted by the forehead. The general field of vision for light was greatly restricted in this region. The average extension of the field of vision, determined by the subject's ability to discriminate light, was similar for all subjects. The average extensions were, respectively M, 77.8; W, 79.3; MH, 75.6; and H, 75.5 degrees.

IV. SUMMARY AND CONCLUSIONS

Under our conditions there was good evidence of peripheral form discrimination. The retinal distribution of light emitted by a form was an important factor determining the distance from the fovea at which it could be discriminated. The triangle had an average field of 55°, while a hexagon of the same area and brightness had a field of only 26°. The rectangle, square, diamond, and circle had fields, respectively, of 35°, 46.5°, 46°, and 44.5°. Analysis of the errors of report shows that the triangle was most accurately and the hexagon least accurately reported.

The extent of the form field was greater on the horizontal than on the vertical meridian, but there were more erroneous reports on the horizontal than on the vertical meridian. The average extension of the field of vision for light alone ranged from 75.5° to 79.3° for our four subjects. The field of vision was restricted in the forehead region.

REFERENCES

1. AUBERT, H. Physiologie der Netzhaut. 1865, 240
2. FERREE, C. E., & RAND, G. A new laboratory and clinic perimeter. *J. Exper. Psychol.*, 1922, **5**, 46-67.
3. FERREE, C. E., RAND, G., & MONROE, M. M. A study of the factors which cause individual differences in the size of the form field. *Amer. J. Psychol.*, 1930, **42**, 63-71.
4. GEISSLER, L. R. Form perception in indirect vision. *Psychol. Bull.*, 1926, **23**, 135-136.
5. KLEITMAN, N., & BLIER, Z. A. Color and form discrimination in the periphery of the retina. *Amer. J. Physiol.*, 1928, **85**, 178-190.

6. Lashley, K. S. The mechanism of vision: I. A method for rapid analysis of pattern vision in the rat. *J. Genet. Psychol.,* 1930, **37**, 453-460.
7. Munn, N. L. A note on Lashley's method for studying vision in the rat. *J. Genet. Psychol.,* 1930, **37**, 528-530.
8. Zigler, M. J., Cook, B., Miller, D., & Wemple, L. The perception of form in peripheral vision. *Amer. J. Psychol.,* 1930, **42**, 246-259.
9. Wertheim, Th. Über die indirekte Sehschärfe. *Zsch. f. Psychol.,* 1894, **7**, 172-187.

University of Pittsburgh
Pittsburgh, Pennsylvania

CAN GESTALT THEORY SAVE INSTINCT?

M. Bullard Drury

The concept of instinct has long been an important one in the psychologist's attempt to understand the functioning of living organisms. In recent years, however, this concept has been the target for considerable criticism and there have been vigorous proposals that it be given up entirely. But the validity of the concept has sometimes been confused with the validity of particular theories of the mechanism or bodily devices underlying instinctive behavior. Since the concept has developed along with a particular notion of the manner of functioning of the organism, this confusion is understandable. Quite recently a new theory of the way the organism functions has been gaining ground—a theory that has been emphasized especially by the Gestalt school of psychology. We believe that this theory suggests a view of instinct which is not open to the objections that have been raised against other doctrines.

The *Gestalt* theory of the functioning of the organism has been explicated in most detail by Köhler in his *Gestalt Psychology* (3). According to him, the essential basis of our conduct is brain events whose distribution and organization are determined, not by neural pathways along which impulses have come, but by the mutual influences of these processes upon each other in accordance with the dynamic principles of physical chemistry. The total brain events are physico-chemical systems which tend to a state of dynamic equilibrium. By dynamic is meant that there are events continually going on in this system. The system is said to be in equilibrium because the resultants of such events remain constant. Let us take a simple example. Consider a jar of water into which is poured more sugar than can be dissolved. Particles of sugar are continually dissolving and particles continually coming out of solution into a solid state. For a time more sugar dissolves than crystallizes. The system is not in equilibrium. Finally, the amount of sugar in solid form does not change, for now the amount dissolving is equal to the amount coming out of solution. The solution is said

to be saturated and the system to be in equilibrium. It is a *dynamic* equilibrium because events are going on in the system; it is an equilibrium, because the resultant remains constant; it is a *system* and not a mechanism, because the behavior of every particle in the total system depends upon the influence of all the rest (6, pp. 127-130; 1, p. 391).

In the case of the human organism, the system is made up of the entire body, certain parts of which, namely, the neuro-muscular system, are the most important in behavior. In unlike acts of behavior certain parts show more marked activity than others and so somewhat different systems may be said to be involved in these acts. Now it may be asked, how can such systems be responsible for behavior when the organism is stimulated? It may be supposed that a stimulus is merely some event which disturbs the equilibrium of a certain system. Therefore events occur which tend to re-establish the equilibrium. The behavior which results is merely the external consequence of these events. (How the muscular system is connected with the part of the nervous system where the dynamics go on need not be considered here.) When equilibrium has been re-established the conduct will cease. The variety of conduct possible to an individual in various situations will therefore depend upon what systems he possesses, the equilibrium of which can be disturbed, and also upon the positions of equilibrium for these systems.

According to this view of the functioning of the organism, the somewhat figurative expression "the end determines the means" becomes neurologically understandable. The end is the position of equilibrium finally attained; the means are the events that will take place until equilibrium is attained. The tendency to gain equilibrium may be considered the drives motivating the organism. The positions of equilibrium will determine the direction of these drives, for all that the organism does is but a consequence of disturbed equilibrium. When that disappears, the incentive for conduct disappears.

In order to understand the functioning of such an organism, it is necessary to know the nature of its systems at birth, their positions of equilibrium and the way changes in these systems can come about. By instincts are meant the systems which the organism has at birth. The various instincts will be specified by a statement of the equilibrium-positions of the corresponding systems. It is obvious that these facts must be known; for how is it possible to understand the organism if its nature at the earliest moment that it can be examined is not known?

This view of instinct follows naturally from Köhler's doctrine of the functioning of the physiological organism. Although it seems to differ slightly from others which have been proposed by the Gestalt psychologists, it may be that the difference is only one of terminology. Both Koffka's and Ogden's treatment of instincts imply a similar organic functioning. In Ogden we find a characterization of instinct in terms of patterns which

include the situation and the response. He emphasizes the total pattern of behavior "with its enclosure of beginning and end" (5, p. 19). His interest, from the standpoint of education, is not in a discussion of the physiological correlates of behavior. Koffka mentions that "physiological processes take place both in the form of 'closed' and 'unclosed' responses" (2, p. 98). His exposition follows logically from Köhler's exposition of the characteristics of *physischen Gestalten.* Köhler himself rejects the chained-reflex theory as being impossible and he emphatically rejects the theory which regards instinct as a supernatural process (3, p. 388); but he does not formulate any theory of instinct based upon his dynamical principles.

A problem closely related to that of instinct will be that of learning. How do changes in these inherited dynamic systems come about? As soon as the individual is exposed to stimuli and situations capable of disturbing the equilibrium of his systems, the systems will operate in the ways suggested. As they do so under various conditions they will undergo change—a change usually referred to as learning. The important question will be: Are the changes that take place changes in the positions of equilibrium or in the resources of the individual for attaining these positions of equilibrium? It is possible that both kinds of change may occur.

A further question may be raised about instincts. May there not be systems, not present at birth, which occur later in the life of the individual but not as a consequence of modification of any pre-existing systems; that is to say, may there not be systems developing as a result of maturation? If so, this would mean that the definition of instinct should be revised to take account of systems which may develop by maturation. Instincts, then, are the neurological systems which are present at birth or appear as a result of maturation, that is to say, they are not a consequence of learning. Whether there are any such maturated instincts and what they are cannot be decided beforehand. The notion merely offers an additional possibility to the psychological theorist. When he finds that the direction of an individual's conduct is determined by some position of equilibrium which cannot be explained as a result of the modification of pre-existing systems, he may find an explanation in systems which have just come into a functional state of development. The idea thus offers him a guide for investigations, not a ready-made dogma.

It may finally be asked how these positions of equilibrium are to be specified. For practical purposes it is necessary only that empirical every-day designations of the resulting conduct be given. Ultimately the underlying neural systems and their positions of equilibrium should be described in physiological terms.

We may now survey the criticisms that have been raised against previous doctrines of instinct and determine whether they are applicable to the present doctrine. Tolman (7), writing in 1923, listed the chief criticisms made up to that time. They are summarized by him under five heads: (1)

non-variability, (2) arbitrary grouping, (3) "pre-established harmony" and "innate ideas," (4) "faculty psychology," and (5) fundamental instinct-habit identity.

(1) The charge of non-variability calls attention to the extreme flexibility of so-called "instinctive" behavior. In the nest-building of birds, for example, the bird at various times follows different sequences of performance. In all cases it gets the nest built and maintains its activity until the nest is built, but not by means of a rigid and identical set of movements in the various cases. Pure reflex-pattern theories imply that acts occur in a definite and rigid sequence. This type of theory, according to Tolman, has been discredited by the charge of non-variability and he believes the indictment to be sound. He does not believe, however, that it discredits the simple-reflex type of theory. In this type of theory only simple reflexes are considered as inherited and thus instinctive. The patterns are formed by learning. But even though it is learning, and not inheritance, that combines the simple reflexes into patterns, still, since the components are reflexes the patterns should be rigid once they are combined. So even the reflex theory seems to imply non-variability and is open to the criticism that real behavior does not show it. This criticism is not applicable to the doctrine proposed here. According to it, the only thing that need be constant in the performance of a given instinctive act at different times is the end that is reached—the final position of equilibrium that is attained. It is not necessary that this be accomplished by the same sequence of movements every time. Any movement may occur that contributes to the attainment of the final equilibrium. Since the total organism would be likely to be somewhat differently adjusted at different times, we should, in fact, expect that the sequence of movements which would lead to a final equilibrium would be correspondingly different. The present doctrine, far from implying a rigid sequence of movements, implies just the opposite.

(2) The criticism of arbitrary grouping asserts that instincts are arbitrarily classified according to the special interests and purposes of individual authors. This charge is obviously a criticism of classifications of instinct and not of the concept itself or of any proposed mechanism of instinct. The criticism does not state that there are no instincts, but merely that certain difficulties are encountered in the classification of them. This classification, according to the present proposal, would depend upon the particular positions of equilibrium that were discovered. These could be discovered only by observation of the conduct of the individual and so a consistent classification would rest on the same basis as that of other experimental problems.

(3) "Pre-established harmony" or "innate ideas" is a charge which consists in "declaring that instinct-doctrines imply a sort of Leibnitzian pre-established harmony between the organism and its environment" which has endowed the organism with an instinctive equipment capable of meeting its needs in the environment. The instincts are supposed to appear before the

organism has acquired any experience of, or acquaintance with, the objects of its environment, and so instinctive behavior implies a pre-established relation between the organism and its environment. Tolman does not deny the implication of a pre-established harmony, but tries to justify it by pointing out that it is the essence of what evolution is supposed to accomplish. That is to say, the fitness of the animal for its environment is explained by its evolutionary history. Tolman may be correct in admitting that previous doctrines of instinct do imply the existence of a pre-established harmony. We do not see, however, that the present doctrine is open to such a charge. All that it contends is that when the equilibrium of a certain physiological system is disturbed, events will occur in the organism which tend to re-establish that equilibrium. The behavior will be appropriate to the exciting situation because the situation, to the extent that it is exciting, arouses a corresponding state in the organism, i.e., a particular state of disequilibrium. The behavior which finally re-establishes equilibrium in the organism will, consequently, be nicely attuned to the situation which aroused it.

(4) Teleological theories, which is the name Tolman uses for those theories which characterize instincts in terms of their purposes or ends, are said to assume mystical drives or forces behind the actual responses and to be open to the same criticism that is made of faculty psychology. These theories add nothing to a causal and descriptive explanation of the phenomena. Tolman says, however, that the assumption of instinctive "faculties" is, on purely methodological grounds, not only permissible but actually demanded, for it simplifies and facilitates causal (i.e., functional) explanations. Tolman may be right in some cases and not in others. (*a*) If the teleological theory in describing the instinct by reference to its end is intended only as a description, then no faculties are implied and the criticism is invalid. (*b*) If the teleological theory invokes a faculty to explain the instinct in the sense of saying that there is some underlying bodily basis, then Tolman's justification is correct. Such a doctrine merely points to the need of finding a cause. (*c*) If the teleological theory states that it has explained the behavior by calling it instinctive, then the criticism that compares this procedure to that of faculty psychology is valid and Tolman's defense is not. The present doctrine would probably be classified by Tolman as a teleological theory, for it states that the conduct that ensues when an instinctive system is disturbed is most appropriately characterized in terms of the end to be attained. It does not, however, assume non-observable powers or faculties to explain such behavior, but suggests a concrete *physiological* device which accounts for the continuance of the behavior until a certain end is reached. There is, moreover, no implication of conscious purpose in the attainment of a particular end by the organism. It is questionable, therefore, whether the present theory may be appropriately characterized as teleological.

(5) The next criticism, which is called "instinct-habit identity," denies any fundamental difference between habit and instinct. The argument takes two forms. The one type of argument is that habits are reducible to instincts, the other is that instincts are reducible to habits. This criticism is not relevant here because the present doctrine does not posit two different kinds of *behavior,* but considers the total organism as involved in all cases. Behavior itself does not bear the stamp of being original or acquired. The position taken is, rather, this: The organism at any time is made up of physiological systems that are responsible for its conduct. These systems are a function of what they were at birth or as a consequence of maturation, *and* of the changes that may have been produced in them by use (changes commonly referred to as learning). We should not expect that all such functional systems could ever be divided into two sharply divided classes, instincts and habits. If a classification of these systems on the basis of their degree of modification were desired, we should propose instead a continuous series. At one end would be represented those systems which we have called instinctive—those which had not undergone modification. At the other extreme would be represented those systems which had undergone the maximum amount of modification; and continuously along the line, those which had undergone intermediate degrees of modification.

Another criticism must now be added to Tolman's list due to a recent "anti-heredity movement" headed by Kuo (4) and summarized by him in 1929. This charge is that, because instincts are defined in terms of heredity, and because heredity is not experimentally approachable and cannot be used for differentiating behavior, instincts as well as heredity must be given up by psychology. These problems, according to Kuo, are at bottom biological problems. "We can accept the organism as given and start to investigate its behavior in response to environmental stimulation without reference to heredity. We need not ask how the organism comes about, or how heredity determines the organismic pattern. This is a question the biologist must answer." The criticism, obviously, rests upon the limits and scope of the problems of the psychologists. If, as certain students of the subject contend, stimulus and response constitute the entire field of psychology, then necessarily they will not be concerned with broader aspects such as the genesis of the organism or any of the physiological systems. Kuo's argument does not destroy the problem, it only places it in another field.

The present view of the nature of what is usually called instinct seems, then, to meet the objections that have been applied to other doctrines.

References

1. Getman, F. H. Outlines of theoretical chemistry. New York: Wiley, 1927. Pp. xiii+539.
2. Koffka, K. The growth of the mind. New York: Harcourt, Brace, 1924. Pp. xvi+383.

3. Köhler, W. Gestalt psychology. New York: Liveright, 1929. Pp. x+403.
4. Kuo, Z. Y. The net result of the anti-heredity movement in psychology. *Psychol. Rev.*, 1929, **36**, 181-199.
5. Ogden, R. M. Psychology and education. New York: Harcourt, Brace, 1926. Pp. xiii+334.
6. Smith, A. General chemistry for colleges. New York: Century, 1916. Pp. x+662.
7. Tolman, E. C. The nature of instinct. *Psychol. Bull.*, 1923, **20**, 200-218.

Cornell University
Ithaca, New York

CHANGES IN SKIN POTENTIALS DURING THE PSYCHOGALVANIC REFLEX

James W. Broxon and Karl F. Muenzinger

The purpose of this study was to investigate with a quadrant electrometer the changes in skin potentials during the psychogalvanic reflex. This static instrument, which has not been used for this purpose before as far as the authors are aware, has the advantage that it measures potential differences without any current flowing through the electrodes, thus permitting continuous readings that are unaffected by changes in resistance and polarization.

That changes in body potentials occur as primary electrical phenomena during the reflex has recently been denied by Wechsler (5) and reaffirmed by Jeffress (3), who shows that changes in body potentials occur even after the initial potential has been "neutralized by an external potential obtained from a potentiometer," a condition which would exclude the explanation of the Tarchanoff phenomenon (i.e., the phenomenon when there is no external source of E.M.F. used) through changes in the counter E.M.F. of polarization as suggested by Wechsler. Our own observations fully confirm Jeffress on this particular point. We should like to point out, however, that such a balance of potentials as he used is naturally a very temporary affair, difficult to obtain, and that a current does flow as soon as the balance of potentials is disturbed. And whenever a current flows there are the added factors of resistance, polarization and its counter electromotive force to be taken into account. It would, therefore, seem desirable to investigate this phenomenon under conditions such that there is at no time a flow of current. This is possible with the quadrant electrometer which leaves the electrodes on the skin at all times in an open (external) circuit.

There is, of course, the inappreciable transient charging current of the quadrant electrometer. It should be borne in mind, in this connection, that

it is not the current at a particular instant but the quantity of electricity passing in a given direction which determines the amount of polarization. The quantity of electricity required to account for a deflection of the electrometer as observed in this investigation is exceedingly minute in comparison with the amount which must pass through a sensitive galvanometer while it is being read. Thus the electrometer we used, an instrument of about 20-cm. capacity designed by W. F. G. Swann, when operated at a sensitivity of only 1000 divisions per volt, would be deflected through one division by a quantity of electricity of the order of $20 \times 10^{-12} \times 10^{-3} = 2 \times 10^{-14}$ coulomb. To maintain for one second a deflection of one division on a galvanometer of a sensitivity of 10^{-9} ampere per division would require 10^{-9} coulomb. Hence to maintain the unit galvanometer deflection for one second there would be required about 50,000 times as great a quantity of eelctricity as is required to produce a unit electrometer deflection. Moreover, the electrometer deflection may be maintained indefinitely with no further passage of electricity, whereas the amount of electricity passing through the galvanometer is directly proportional to the time during which the deflection persists. In this connection it may be mentioned that the capillary electrometer, such as used by Radecki (4), is a closed-circuit, low-resistance instrument, and has, therefore, the same disadvantage as does a galvanometer so far as the resistance and polarization are concerned.

The Experiment

Seven subjects were used, five men and two women. The stimuli for the reflex consisted of stimulus words to which the subjects had to respond in the usual fashion, problems in arithmetic, and infliction of pain.

The electrodes used were nickel plated copper disks, 2.5 cm. in diameter, except where otherwise noted.

The electrometer deflections were read visually in the usual manner by means of a mm. scale at a distance of one meter from the instrument.

The results of the various parts of the experiment were as follows:

Initial Deflection. When the electrodes were attached to a subject there was an initial deflection which did not remain constant. To cite only two instances: With one electrode attached to the thenar eminence and the other to the ulnar ridge of the palm of one hand the initial potential difference in one case was .032 volt, changing slowly to .007 volt during the course of 20 minutes; in another similar case there was a change from .044 volt to .067 volt.

The direction of this deflection seemed to be determined in part by the relative amounts of moisture on the two electrodes, the one with more moisture always being positive in relation to the other. Either electrode could be made more positive by squirting some water between it and the skin.

Pressure also influenced the initial deflection. Moderate pressure on

one electrode always tended to make it positive in relation to the other. When the experimenter started with a mere touch on one electrode there was sometimes an initial very slight deflection in the opposite direction which reminded one of a similar deflection which often follows immediately upon a stimulus. Increase of pressure always brought about the positive deflection. Finally, with extreme pressure this deflection was often reversed again. The amount of pressure required to reverse the potential difference appeared to depend upon the material of which the electrodes were made.

These facts indicate that *pressure on the electrodes, which one naturally associates with a change of resistance, also affects the potential difference between the electrodes when no current is flowing.* Moreover, they seem significant in indicating what the physiological nature of the psychogalvanic reflex is, although it appears to the authors that further investigation in this direction is needed before one may propose a definite physiological theory.

Deflections during Response to Stimulation. Upon stimulation, the same sort of deflection was observed as is usually observed with a galvanometer in series with the electrodes and the body. During the latent period there was often a slight dip in a direction opposed to the regular deflection, the *Vorausschlagphänomen* of Veraguth. After the regular deflection had occurred there was also the usual tendency to return towards the starting point.

The direction of these stimulus deflections was sometimes the same as that of the initial deflection and sometimes opposed to it. It usually stayed the same, however, during one set of readings. The attempts to relate the polarity of the deflections to such characteristics of the skin areas as relative profusion of sweat glands and moisture and to relative degrees of pressure exerted on the electrodes has led to no definite conclusions so far. Experiments on this moot point are being continued.

The magnitude of the deflections during a set of 25 readings indicated an average change of potential differences of .005 volt. The range was 0 to .017 volt.

Comparison of Electrometer Readings with Simultaneous Readings of a Closed Circuit Instrument. In order to test the relation between changes in skin potentials and resistances the electrometer was attached to one hand of the subject (thenar eminence and ulnar ridge of palm) and an apparatus developed by S. R. Hathaway to symmetrical areas on the other hand, and readings were taken simultaneously.

In the latter apparatus the electrodes are in an alternating current circuit (60-cycle) in which the effective current changes are due to alterations in resistance at the electrodes or in the body. By means of a transformer these changes are impressed upon the grid circuit of two vacuum tubes and the amplified changes in the plate current are read as deflections of a milliammeter in the latter circuit. Whether or not the changes in

resistance observed by this apparatus are only apparent and are really changes in counter E.M.F. of polarization must be cleared up by further work. The investigations of Galler (1) showed that with an alternating current of several thousand alternations per second the reflex could not be noticed, while the work of James and Thouless (2) showed that the effect of polarization with an alternating current of 50 alternations per second is still appreciable. Both investigations would indicate that the changes observed by the Hathaway apparatus are not changes in real resistance.

Care was taken to see whether the starting up of the alternating current circuit would influence the electrometer (with the electrodes connected as noted above). At no time could any influence be noted. The electrometer system being entirely insulated from earth was found to behave exactly as when used alone.

The simultaneous deflections of the two instruments were very similar in character and magnitude. One series of 25 readings yielded a correlation (rank difference) of .86±.038, and another .89±.027. Jeffress (3) obtained correlations of .95 when he used two circuits, one with and one temporarily without a current flowing through the body due to an external source of E.M.F. Our correlations compare favorably with his if one considers that our readings were taken by the two experimenters, one observing only one instrument; that the A.C. circuit varied slightly due to changes in the supply line voltage; and that the deflections were comparatively small and of short duration. The fact that our correlations were obtained between readings of a closed-circuit instrument with alternating current and those of an open circuit instrument without any current probably has nothing to do with their magnitude.

Summary

1. When a static instrument is used to indicate potential difference, an arrangement which leaves the electrodes at all times in an open circuit, the psychogalvanic reflex exists and is evidenced by changes in the potential differences between the electrodes, giving deflections that are similar in character to those observed commonly with closed-circuit instruments.

2. Changes of potential differences during response to stimulation are highly correlated with changes in resistance (real or apparent) as measured with an alternating, 60-cycle, current.

3. Moisture and pressure are factors that affect the initial potential difference when solid electrodes are used.

References

1. Galler, H. Über den elektrischen Leitungswiderstand des tierischen Körpers. *Arch. f. d. ges. Physiol.*, 1912, **149**, 156-174.

2. James, H. E. O., & Thouless, R. H. A note on the effect of polarization in psycho-galvanic experiments. *Brit. J. Psychol.*, 1926, **17**, 49-53.

3. JEFFRESS, L. A. Galvanic phenomena of the skin. *J. Exper. Psychol.*, 1928, **11**, 130-144.
4. RADECKI, W. Recherches expérimentales sur les phénomènes psycho-électriques. *Arch. de Psychol.*, 1911, **11**, 209-295.
5. WECHSLER, D. The measurement of emotional reactions. *Arch. Psychol.*, 1925, No. 76.

University of Colorado
Boulder, Colorado

APPARATUS

A MECHANICAL MODEL OF THE CONDITIONED REFLEX[1]

H. D. Baernstein and Clark L. Hull

It is a plausible hypothesis, though one by no means yet established, that the conditioned reflex is the basic mechanism of the more complex adaptive mammalian behavior. This fact gives it great possible significance. The strikingly mechanical appearance of its behavior offers a challenge to the construction of models which shall display similar characteristics. The synthetic approach typified by the construction of such a model is in line with a well-established tradition in scientific research.[2]

To a certain extent the model here reported parallels the reacting organism. Push buttons represent sense organs or receptors. A small electric flashlight bulb (2.5 volts) represents the responding system. Copper wires serve as nerves. The synapses of the nervous system are represented by mercury-toluene thermoregulators (Figures 2 and 3). Changes in small storage cells made from two lead plates suspended in sulphuric acid represent some of the chemical changes occurring in the nervous system. Electric batteries supply the energy. No claim is made that these mechanisms are duplicates of the corresponding organic processes. It may not be without significance, however, that so many of the properties of the conditioned reflex can be paralleled by a polarizable cell.

Figure 1 shows a diagram of the electric circuits employed, in relation to the other parts of the model. In this diagram, S_u represents the unconditioned stimulus, the S_c's the several conditioned stimuli, and S_x the extra stimulus. Several of the push-button receptors involve multiple contacts. To avoid ambiguity each is surrounded by a dotted rectangle. The C's represent the conditioners, and D the differentiator. The heating wire is coiled around these units instead of being at a distance as the diagram might suggest. The E's are the extinguishers—small storage batteries. R is a relay and L_r the responding light bulb.

Simple Stimulus Substitution and Forgetting

Pavlov found (3, pp. 33 ff.) that whenever food is placed in a dog's mouth (S_u), saliva will flow. This is called an unconditioned (inborn)

[1]The germinal idea and basic plan of the present project originated with the second author; the design and construction of the mechanism is due entirely to the first. A preliminary note (2) appeared in *Science*, 1929, **70**, 14-15. The model was exhibited at the Urbana meeting of the Midwest Psychological Association in May, 1929.

[2]Lillie's famous iron "nerve" is an example from a closely related field.

reflex. If on a number of occasions a bell is sounded (S_c) just before the presentation of the food, the bell acquires the capacity independently of evoking the flow of saliva. S_c thus becomes substitutable for S_u and the reaction (L_r) is said to have become conditioned to S_c.

A study of Figures 1 and 2 shows how stimulus substitution is accomplished by the model. If S_u is pressed at any time, the bulb will become luminous because the closure of the first pair of contacts throws the 2-volt current

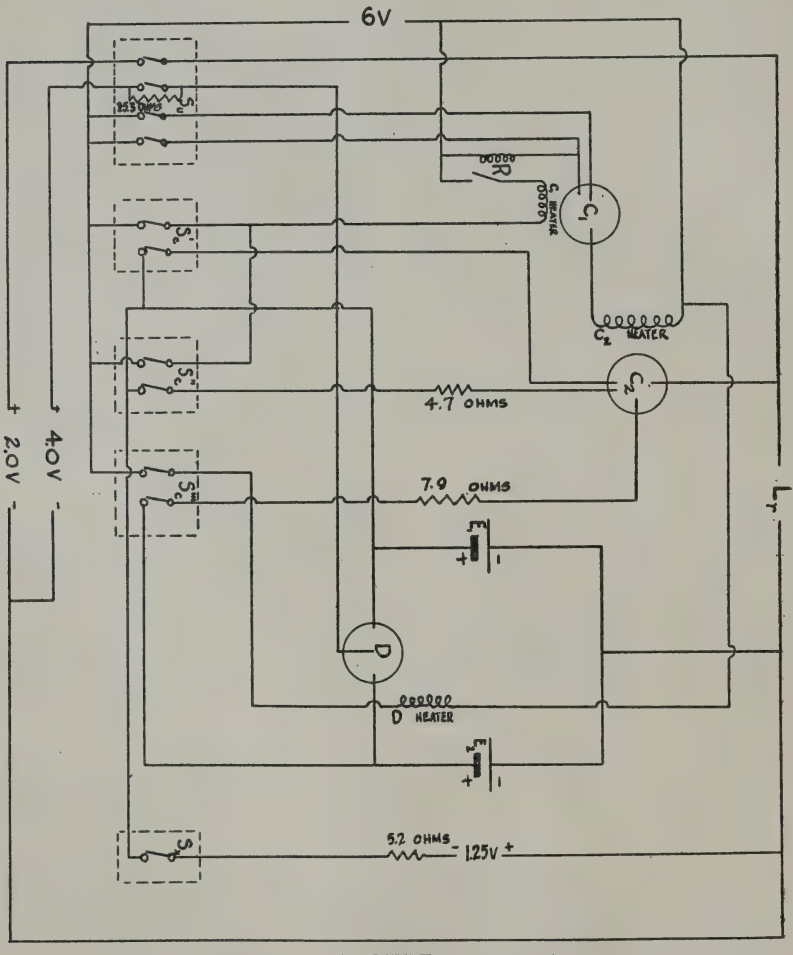

FIGURE 1
WIRING DIAGRAM OF MODEL

through L_r. This represents the unconditioned reflex. But if any of the S_c's are pressed there will be no observable response. Now if S_c' be pressed for a few seconds, the closure of its first pair of contacts sends a current through the heating wire surrounding the tube of toluene of C_1, the circuit at R being normally closed. The expansion of the toluene in the sealed

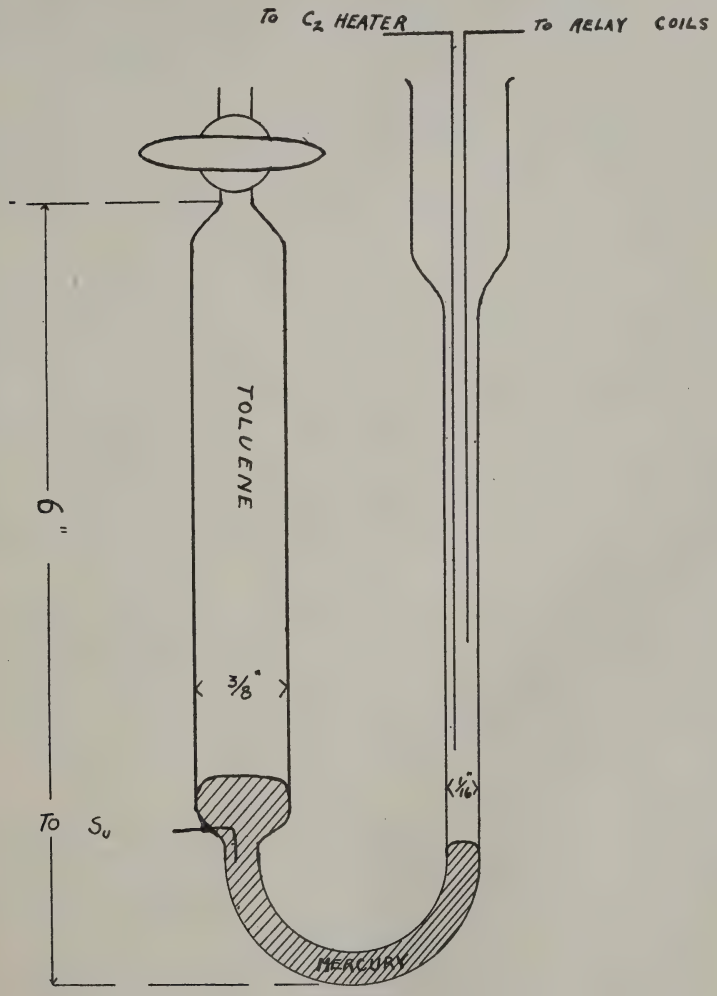

FIGURE 2
CONDITIONER C_1

chamber presses the mercury downward (Figure 2) and thence up into the capillary until it makes contact with the first wire.[3] After this has taken place a pressure on S_u (through the closure of the third pair of contacts) will send the 6-volt current through the heating wire surrounding C_2. This, in turn, causes the mercury to rise in the capillary tube of C_2. When the mercury has risen to the point where it makes contact with the first wire, there is established a circuit such that when S_c' is pressed its second pair of contacts will send a current from the storage cell E through L_r which becomes luminous. The stimulus substitution has been accomplished. The result corresponds to the conditioned reflex.

If the apparatus is left undisturbed for a time, the toluene column cools, permitting the mercury to fall in the capillary tube and thus breaking the circuit made by closing the second pair of contacts of S_c'. Consequently pressing S_c' will no longer evoke the reaction. Since time has produced this effect it corresponds, in a way, to forgetting.

The Order of Stimulation to Produce Conditioning

Available experimental evidence[4] indicates that while conditioning may take place with any close temporal relationship of the conditioned and unconditioned stimuli, the process goes on most readily when the unconditioned stimulus precedes the one to be conditioned. An attempt was made to incorporate this principle into the model. As already pointed out, conditioning cannot take place until both C_1 and C_2 have been heated, the former by S_c' and the latter by S_u. Moreover, C_2 cannot begin heating until C_1 has first been heated. But, since S_c' heats C_1 and S_u heats C_2, stimulation of S_c' must precede stimulation of S_c before the latter may be effective in the conditioning process. Thus an approximation to forward conditioning is effected.

If, at the beginning of the conditioning process, S_u and S_c' are pressed simultaneously, the fourth pair of contacts of S_u will send a current through the coil of the relay R. This opens the related switch so that S_c' cannot heat C_1. Simultaneous conditioning is thus prevented. Since simultaneous conditioning actually takes place very readily, this part of the model might be eliminated by removal of the fourth pair of contacts of S_u.

---

[3]It should be noted that C_1 is so constructed that when the mercury has risen in the capillary tube to a height such that it makes contact with the second wire, the new connection shunts part of the current through the coils of the relay. This operates the relay and breaks the circuit (condition shown in the diagram) which automatically eliminates the current from the wire encircling the toluene tube and thus prevents a possible overheating of the unit.

[4]Pavlov (3, pp. 27 ff.), basing his view on the experimental work of Krestovnikov, originally held that the conditioned reflex could not get set up if the unconditioned stimulus precedes the conditioned stimulus. The model was built before the authors had seen Pavlov's later work and before Switzer (4) had definitely proven backward conditioning in human subjects.

Experimental Extinction and Spontaneous Recovery

When a conditioned reflex is evoked repeatedly without being accompanied by the unconditioned stimulus, the response is gradually weakened until it disappears. This process is called *experimental extinction* (3, pp. 49 ff.). In observing how this is brought about in the model, it should first be noted that the current which makes L_r glow in the conditioned reaction comes from the small storage cell E_1. This storage cell is charged, in the main,

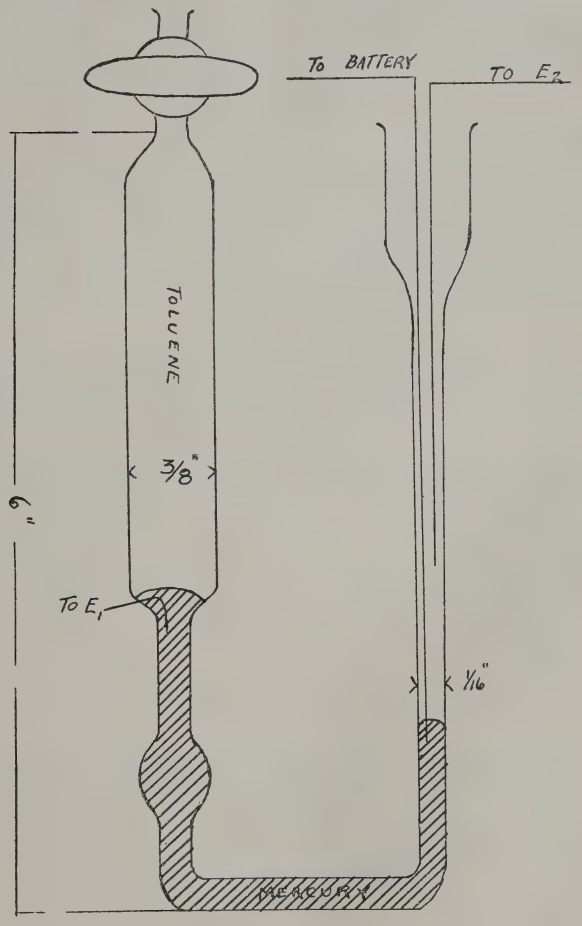

FIGURE 3
Differentiator

from a 4-volt current by the joining of the second pair of contacts of S_u (Figure 1). But with repeated use this charge gradually becomes exhausted and the vigor of the luminous reaction correspondingly weakens and disappears, thus paralleling the phenomenon of experimental extinction.

If an animal with a stable conditioned reflex which has suffered experimental extinction be left undisturbed for a time, the conditioned reflex will spontaneously recover much of its original strength (3, pp. 58 ff.). In the model this is accomplished by the well-known tendency of storage cells, when discharged rapidly, of spontaneously recovering a portion of their original strength if left for a time undisturbed. Under favorable conditions several successive recoveries may be made by such a cell, the amount of current delivered on each occasion being progressively less. This corresponds closely to the diminution in strength of the conditioned reflex with successive extinctions and recoveries.

Owing to the fact that the spontaneous recovery tendencies of the storage cell are not always sufficient to make dependable demonstrations, it is supplemented in the model by an accessory device. The second pair of contacts of S_u are connected by a coil of high resistance wire. This causes a slow charging of E_1 or E_2 even when S_u is not pressed down.

Secondary Extinction

It has been found that if two stimuli of different physiological strengths have been conditioned and the stronger is later extinguished, the weaker one will also show extinction. If the weaker one is extinguished the stronger will survive, but usually in a weakened state. These phenomena are known as *secondary extinction* (3, pp. 54 ff.). In the model, S_c' represents the stronger reflex and S_c'' the weaker. A resistance of 4.7 ohms in the S_c'' circuit permits the bulb to glow less brilliantly when the current passes through it than when it passes through S_c'. Now when S_c'' is closed repeatedly without reinforcement, the bulb at length will fail to respond, but S_c' will still give a positive, though weakened, response. The reason is that E_1 has been so far exhausted of its charge that it is unable to overcome the resistance in the S_c'' circuit sufficiently to make L_r luminous, though it still retains enough energy to evoke the response over the less resistant circuit of S_c'.

If the reflexes now are both strengthened by reinforcement with S_u, S_c' and S_c'' will again give normally positive responses. If S_c' is next pressed repeatedly, the intensity of the luminous reaction will gradually weaken until it is no longer visible. In this case, if S_c'' is pressed, no visible response whatever will appear.

Irradiation

If a conditioned reflex is set up to a tactile stimulus in a dog, it is found that stimulation of other points on the skin adjacent to the point conditioned

will also evoke the response. The strength of the response regularly decreases with the distance from the point originally conditioned. This phenomenon is known as *irradiation* (3, pp. 152 ff.). It is produced in the model by having three wires, insulated except at their lower ends, extending different distances down into the capillary tube of C_2. Each wire is in circuit with a different push button. As the mercury rises, first S_c' shows the conditioning tendency, then S_c'', and finally S_c'''. As the mercury falls with the cooling of the toluene, the conditionings are lost in the reverse order. Resistances placed in the circuits of S_c'' and S_c''' (Figure 1) reduce the luminosity of the reactions progressively according to the distance of the receptor from that originally conditioned.

Differentiation

When a conditioned reflex has been set up in a dog, it usually shows irradiation, i.e., other stimuli will tend to evoke the same response. If some one of these receptors which has acquired the response by irradiation be reinforced consistently, while alternately the others are stimulated but without reinforcement, after a time the conditioned reaction will be evoked only by the stimulation of the receptor which was reinforced. This favored receptor has been differentiated from the others and the process is accordingly called *differentiation* (3, p. 117). The model parallels this process somewhat imperfectly. We have already seen that irradiation will result from vigorous reinforcement of S_c'. Let S_c''' and S_c' be pressed alternately, the former always being reinforced by S_u and the latter not. The joining of the second pair of contacts of S_c''' will heat the toluene in the differentiator, D. This will force the mercury downward until it no longer is in contact with the wire leading to E_1 (Figure 3) which will break the circuit necessary for charging the latter. At the same time the rise of the mercury in the capillary tube joins in a circuit the two wires suspended in it so that E_2 is charged. After E_2 has acquired a charge, the second pair of contacts of S_c''' completes a circuit through E_2 and L_r, evoking the luminous reaction. Meanwhile the charge in E_1 has been exhausted from use, thus effectually extinguishing S_c' and S_c''.

External Inhibition

Conditioned reflexes in dogs may be inhibited by any unusual stimuli such as loud noises, strange odors, and new surroundings. These are called *extra stimuli* by the reflexologists, and their action *external inhibition* (3, p. 43). In the model, S_x represents an extra stimulus. This push button is connected with a small flashlight battery of about 1 volt. The poles are so connected that they oppose the battery charging E_1. A resistance of 5.2 ohms is inserted to prevent the lamp from becoming luminous when S_c and S_x are closed at the same time. Whenever S_c' is pressed, E_1 becomes rapidly discharged, which prevents a positive response when S_c is subsequently closed.

Redintegration of Compound Stimuli

It has been found that if a complex of stimuli impinges on the organism at or about the time that a response is evoked, each component of the stimulus complex tends to acquire the power independently of evoking the response. A somewhat deceptive appearance of this phenomenon may be obtained from the model. If S_c', S_c'', and S_c''' are pressed simultaneously as a compound conditioned stimulus, followed by S_u as reinforcement, the mercury will finally rise high enough in the capillary of C_2 to make contact with all three suspended wires. Consequently, any one of the three push buttons may evoke the conditioned response. This phenomenon is, in reality, the same as described above under irradiation. It would operate quite the same if S_c'' and S_c''' were not involved in the conditioning process. It would be an easy matter, however, by a simple extension of the methods employed in the present model to effect a genuinely redintegrative behavior.

There are a number of characteristics of the conditioned reflex not shown by the model. Among the more important of these may be mentioned the trace reflex and the delayed reflex. Most important, and probably most difficult of all, is the response to patterns of stimuli as distinguished from the unorganized stimulus complexes considered above (1). If efforts to duplicate these latter should be successful the way would seem to be open to the design of mechanisms manifesting a wide range of adaptive behavior considered by many psychologists as involving mind. It is possible that at a not very remote date the concept of a "psychic machine" may become by no means a paradox.

References

1. Hull, C. L. A functional interpretation of the conditioned reflex. *Psychol. Rev.*, 1929, **26**, 498-511.
2. Hull, C. L., & Baernstein, H. D. A mechanical parallel to the conditioned reflex. *Science*, 1929 **70**, 14-15.
3. Pavlov, I. P. Conditioned reflexes: An investigation of the physiological activity of the cerebral cortex. (Trans. by G. V. Anrep.) London: Oxford Univ. Press, 1927. Pp. xv+430.
4. Switzer, St. C. A. Backward conditioning of the lid reflex. *J. Exper. Psychol.*, 1930, **13**, 76-97.

Department of Physiological Chemistry
University of Wisconsin
Madison, Wisconsin

Institute of Human Relations
Yale University
New Haven, Connecticut

A CATHODE RAY OSCILLOGRAPH APPARATUS FOR THE PSYCHOLOGICAL LABORATORY

ROLAND C. DAVIS

APPARATUS

Many problems in present day psychology are best studied with some form of oscillograph. Both the string galvanometer and the moving coil types of oscillograph have been used, but, so far as the writer is aware, the cathode ray oscillograph, in some respects superior to the others, has not previously been adapted for use in the psychological laboratory.

Such use is made possible by the low voltage cathode ray tube designed by J. B. Johnson (4), which is, incidentally, the least expensive form of oscillograph available. This is a flat-topped vacuum tube about 14 inches in height. In its operation "a stream of electrons is projected from one end of the vacuum tube on to a fluorescent screen at the other end where it produces a bright spot. The electron stream is deflected by passing it through an electric or magnetic field, thus causing the spot to travel over a path in which its location with respect to its normal position at any instant is an indication of the direction and intensity of the field at that instant. As the deflecting field is arranged to vary with the phenomenon under observation, the spot moves in accordance with the variations in the phenomenon.

"It is practically free from inertia and resonance effects due to the fact that the moving element consists of a beam of electrons. This absence of inertia and resonance makes the frequency limitation of the instrument lie at least as high as the upper radio frequencies."

This tube was primarily intended for the study of high-frequency alternating currents and other phenomena which repeat themselves or can be made to do so. Frequent repetitions of the same phenomenon would appear to produce a stationary pattern on the top of the tube which may be traced or photographed with a snapshot camera. Gasser and Erlanger (3) and their associates have used the tube for the study of action potentials of nerves, first by causing the phenomena to recur, and more lately (2) getting photographic records of single phenomenon by laying x-ray film on top of the tube, operating presumably in a dark room.

The apparatus to be described has two new features, a means of taking photographic records of single phenomena on ordinary camera film without the use of a dark room and an electrical circuit making possible the measurement of the interval between two such events as stimulus and its response. An amplifier is included in the apparatus for use in the study of physiological action potentials of the order of one millivolt. The whole assembly is portable and can be set up in about 30 minutes.

The oscillograph tube is mounted in the cabinet to the right in Figure 1, and on the panel of the cabinet is a rheostat for regulating the filament

current (and thereby the focus of the spot), and a voltmeter for measuring the voltage applied to the filament. The two boxes to the left of the oscillograph contain B batteries, which chiefly supply the 450 volts supplied to the cathode to produce the spot. As soon as this voltage is cut off the oscillograph screen becomes dark. It is therefore desirable during photographic work that this voltage be applied only when the record is being taken. Consequently this voltage is passed through the secondary of a small relay, the primary of which is activated by a dry-cell and key. This key is held in the experimenter's left hand and closed whenever a record is to be taken. Otherwise the circuit supplying the oscillograph is the same as that described in the manufacturer's bulletin.

Since the oscillograph spot is capable of movement in two coördinates, with a pair of deflector plates to control its movement in each, it is unnecessary to use a moving film to secure a photographic record on which time is recorded. A potential varying regularly with time may be impressed on one pair of plates, and the phenomenon under observation on the other.

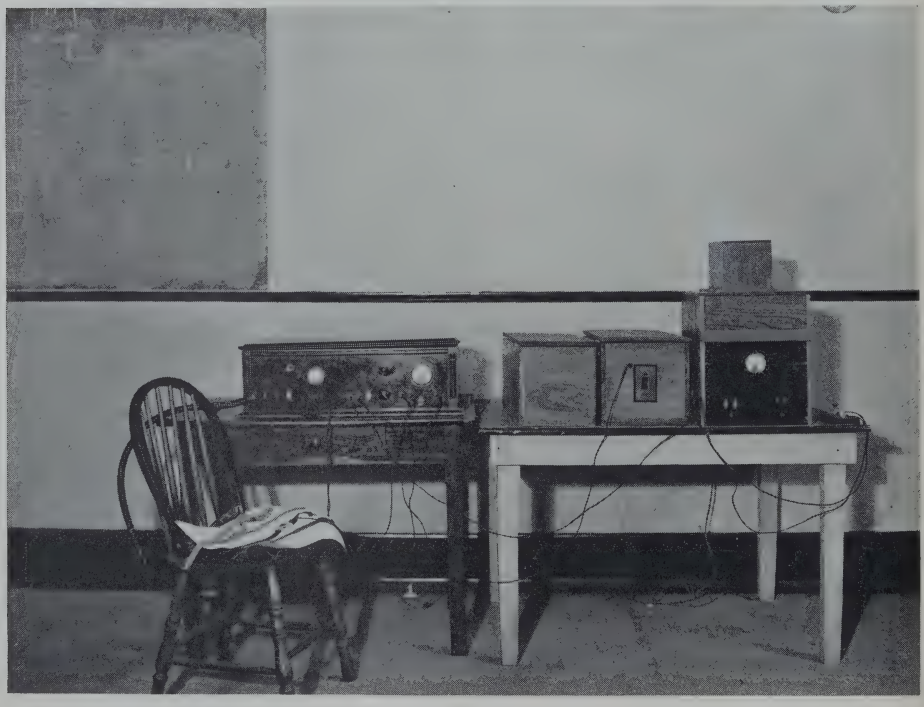

FIGURE 1
AMPLIFIER, BATTERIES, AND OSCILLOGRAPH

In the present apparatus the small box on top of the oscillograph cabinet contains the photographic apparatus. An opening in the top of the cabinet admits the film holder from a Brownie No. 2 camera so that the holder comes to rest on the recording surface of the oscillograph tube and projects about three inches above the cabinet. No lens is used in making the photograph. The film is inserted into the holder inside out and threaded under the guides before each roller. This will bring the emulsion side of the film facing outward at what would normally be the back of the film holder. (The roller, of course, is not wound far enough to expose the emulsion in the daylight.) An iron rod ¼" x 2½", one end fitted with a cross pin and the other end squared, is then fitted into the slot of the driving roller, and the film holder placed inside the box seen in the illustration, with the squared end of the rod projecting through a hole in the box. The back of the holder being down, the film is then wound on to the driving roller by means of an ordinary skate key placed on the squared end of the projecting rod. This brings the emulsion side of the film directly against the top of the tube, since there is enough play in the film to let it conform to the small curvature of the surface, under the weight of the holder.

To economize on the film used records may be taken either horizontally on it, or by setting the tube at an angle, diagonally. The six-exposure No. 120 film will hold a dozen or 15 records diagonally and more horizontally. Under some circumstances a larger size film might be more economical. Although there is no indication of how far to turn the film after each exposure, the matter is not difficult to judge, and a certain amount of overlap of records usually does no harm.

The problems to be met in the timing circuit (potential applied to one pair of deflector plates so as to vary with time) are as follows: (*a*) to supply a potential, if possible, whose variation with time is a linear function, (*b*) to allow the rate of variation with time to be changed readily to suit the phenomenon studied, (*c*) to make the spot start moving coincidentally with the first of the two events (stimulus, for example), (*d*) to avoid the blur of the film produced by having the spot start from a resting position, (*e*) to avoid the production of a large spark in starting the movement, in order that a connected amplifier may function without disturbance. These problems are solved by the circuit represented in the lower half of Figure 2. A varying potential is produced by charging a condenser (*C*) by means of the two-way switch (*S*) (seen on the panel of the amplifier cabinet) and allowing it to discharge across the vacuum tube UX 11. (This feature has been used before.) Since the grid and plate of the tube are connected together, the current passing through them is limited by the emission from the filament and is theoretically independent of anode voltage. Thus the charge on the condenser should decay at a constant rate. This condenser might be charged to a high enough voltage to control the movement of the oscillograph spot directly, but making a connection with such a

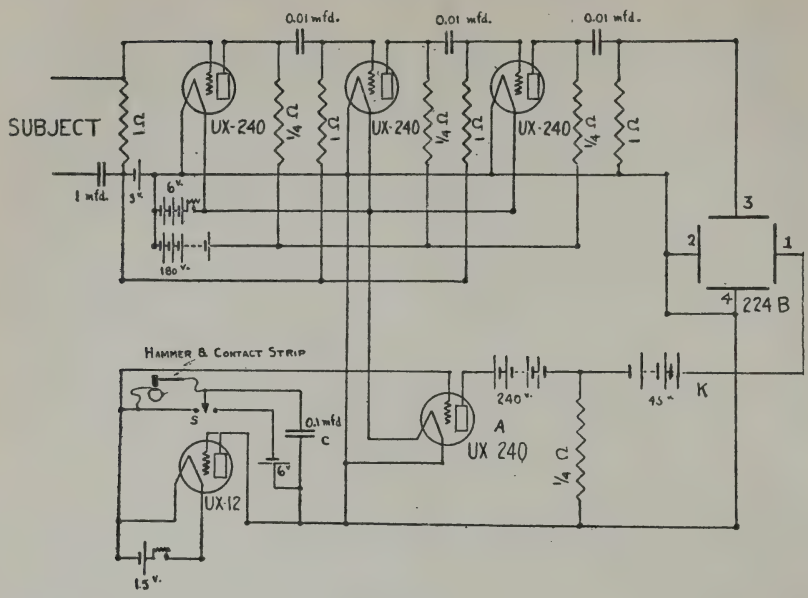

FIGURE 2

AMPLIFIER, TIME, AND OSCILLOGRAPH CIRCUITS

high voltage at the two-way switch would cause too much disturbance in the amplifier when it is used in connection with the oscillograph. Therefore a small voltage is used on the condenser and this voltage is amplified by the tube *A*. This tube is so connected that it receives no voltage from the condenser until a discharge connection is made with the two-way switch or some apparatus in parallel with it. The battery *K* is so placed as to pull the spot toward deflector plate 1, making it stand toward one side of the screen. When the plate voltage of tube *A* is applied, the spot is drawn so far toward deflector plate 2 that it does not appear on the screen at all. When, however, a discharge connection is made at the two-way switch, 6 volts are applied to the grid of *A*, thereby cutting off the plate voltage and causing the spot to jump instantaneously to its position near plate 1. Then as the negative 6 volts decay, the output voltage of *A* increases and the spot moves across the screen at a rate determined by the decay until it comes to rest off the screen. As a matter of fact the decay of this small voltage on the condenser is not linear with respect to time, but this curvature is fortunately corrected over the working range by the opposite curvature of the amplification characteristic of *A*. The rate of discharge is governed by the filament current of UX 11 which is set by means of a rheostat and milliammeter reading to 150, which may be seen on the right of the amplifier cabinet panel.

Thus the spot is made to start, but not from a resting position, at the moment of a stimulus, or other event, and move across the screen with constant and governable velocity. With 360 volts on the oscillograph tube the useful sweep is 50 millimeters. If the tube is used at its rated voltage, (thereby lessening the brightness of the spot) the sweep is about 80 mm. A potential produced in some way by a response or other second event may be applied to the other pair of deflector plates, causing a deflection of the spot at right angles to its line of motion with respect to time.

The Uses of the Apparatus

Employing either part or all of the above apparatus, the oscillograph tube has been put to a considerable number of uses. There are, of course, its uses as an instrument for determining physical constants of psychological apparatus.

It has been employed in this laboratory for the measurement of a series of a.c. voltages impressed across earphones to produce sounds of different intensities. For this purpose only one pair of deflector plates was used, the other pair (otherwise used for timing) being connected together. The earphones were coupled to their oscillator by means of a potentiometer just as they were to be used, and leads were taken to the oscillograph apparatus from the sides of the phones. The voltage was passed through the last stage of the amplifier to increase the deflection. This a.c. voltage caused the oscillograph spot to move back and forth over the same pathway at every cycle, thus producing a stationary line on the screen, which was measured with dividers for the various settings of the earphone potentiometer. The voltage, then, is proportional to the length of this line, and the tube acts as an a.c. voltmeter independent of input frequency. If readings in terms of volts are desired a known voltage may be impressed for comparison. In any case, the relative intensities of sound produced by the several voltages at the phone diaphragm are readily computed from the measurements on the screen.

Purity of the wave form of an oscillator output can be determined either by inspection or by photography. For this purpose the oscillator is con-

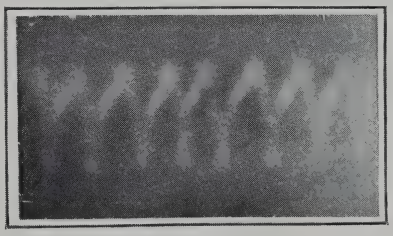

FIGURE 3
Output of 1000-Cycle Oscillator (Impure)

nected to the oscillograph as before, but the timing circuit is allowed to operate on the other pair of deflector plates. A photographic record of a 1000-cycle oscillator output is reproduced in Figure 3. A photographic record of such frequency and spread offers great difficulties for the moving-coil oscillograph.

The tube has not yet been used in this laboratory for comparing and calibrating frequencies of a.c. Its utility for this purpose is clear from such use in physical laboratories. For this purpose the frequency in question is fed to one pair of plates and the standard to the other. The result is a stationary Lissajous figure with a calculable number of points of tangency if one frequency is a multiple of the other.

A use of the tube for measuring the effect of instrumental periodicity in a galvanometer has been described in a previous article (1).

A voluntary reaction-time, or an interval of that order between two stimuli, may be measured in several ways by means of the tube. The best so far developed requires two keys, experimenter's and reactor's, to be placed in series with each other and in parallel with the discharge connection of the timing circuit (where stimulus hammer and plate are shown in Figure 1). An a.c. voltage of 60 or 100 cycles is then impressed on the

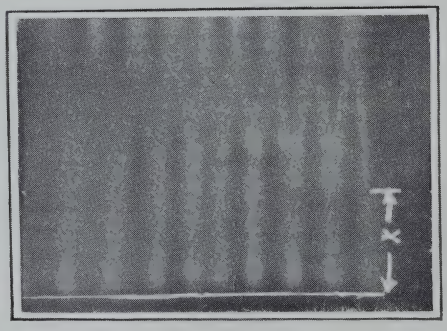

FIGURE 4
REACTION-TIME RECORD

FIGURE 5
SCALE FOR MEASURING REACTION-TIME

deflector plates 3 and 4, the main amplifier being disconnected. Before a reaction the timing condenser is charged, and the reactor depresses his key. The experimenter then depresses his key which may have double contacts, so as to deliver a stimulus of the desired sort, and holds it down. The subject's reaction is then to release his key. When the experimenter's key is depressed the oscillograph spot jumps to one side of the tube and moves back again oscillating as it goes, with the impressed a.c. When the reactor lifts his key, the spot returns immediately to its original position. A record like Figure 4 is produced. The duration of its course can be determined accurately, since its motion in both coordinates has a known relation to time. The number of complete cycles is first counted and multiplied by the duration of a cycle. Then the remaining fraction of a cycle (x in the figure) is measured at right angles to the tangent. The time taken for the spot to traverse this distance is, of course, proportional to $\sin^{-1}x$. A convenient way of determining this is by the use of Figure 5, which is constructed by laying off equal distances on the circumference and erecting perpendiculars to these points from the diameter. Distances on the base line represent equal time intervals, and the whole line, for 60-cycle voltage, represents half a cycle, or 8.33 milliseconds. Thus, even at the ends of the line, readings may be taken to half a millisecond. The reading on this scale is divided by 16, multiplied by 8.33, and added to the time obtained by counting cycles.

The accuracy of this method is limited only by the accuracy of reading the film, since it does not involve the balancing of electromagnets nor the requirement of making one circuit and breaking another at the same time.

An important field open to investigation with this apparatus is that of action potentials and their relations. For this purpose the whole circuit as diagrammed in Figure 1 is employed. The amplifier consists of three stages of the common resistance coupled type which may be expected to respond equally over a wide range of speeds. It has a linear characteristic and a measured voltage amplification constant such that one millivolt produces 5 millimeters deflection of the oscillograph spot. It is not possible to operate the amplifier and the oscillograph tube from the same storage battery without producing a short circuit. The input is impressed across a grid leak of 1.0 megohm, the shunting effect of which can be neglected for most purposes.

In taking records of the electric potential of the heart (Figure 6), one electrode was placed on the chest and one on the arm, the electrode being copper plate faced with saline soaked flannel. The timing condenser was then charged and discharged, thereby recording one or two pulsations. This method, of course, has its limitations, since it allows the measurement at most of only one inter-pulse interval. It does allow the study of the wave form of the pulse potential.

The instrument is best suited for the accurate recording of non-recurrent waves, such as, for instance, the action potentials of tendon reflexes. For

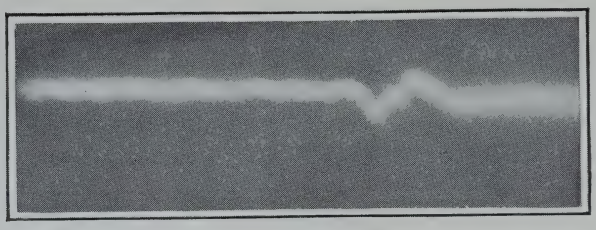

FIGURE 6
ELECTROCARDIOGRAM

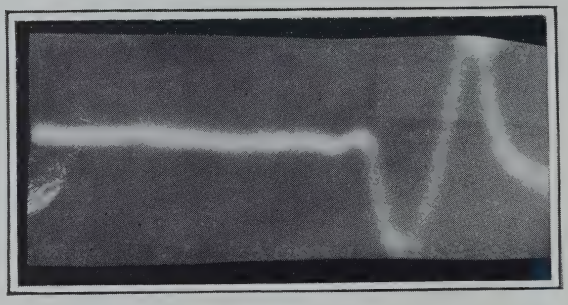

FIGURE 7
ELECTROMYOGRAM

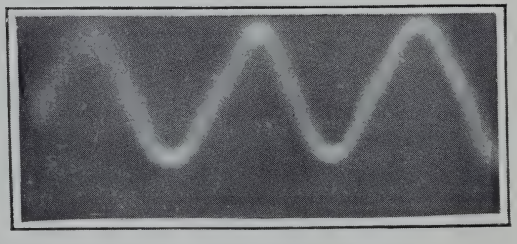

FIGURE 8
60-CYCLE VOLTAGE AS TIME WAVE

this purpose leads are carried to the subject's reacting muscle through the grounded metal tube seen in the photograph. This reduces interference considerably. A metal reflex hammer and a metal plate are connected in parallel with the discharge switch as in Figure 1. This plate is strapped over the tendon and a blow on the plate both sets off the reflex and starts the oscillograph timing circuit. Before each stimulation the timing condenser must, of course, be charged. It is necessary to maintain contact between hammer and plate until the reaction has occurred. When the action potential of the reflex occurs it appears in its appropriate place on the path of the spot across the screen, producing a record like that of Figure 7, an accurate representation of the wave form of the reflex in its proper time relations. In order to determine the rate of motion of the spot across the screen so that the times may be expressed in seconds, records of reflexes may be interspersed with photographs of a known alternating current, taken under the same conditions. On the apparatus pictured a two-way switch on the box to the left of the oscillograph cabinet disconnects the amplifier and throws onto the deflector plates 60-cycle voltage from a transformer connected to the city line. This produces a photograph like Figure 8, showing that the velocity of the spot is 1.277 millimeters per millisecond.

Without doubt, further uses of the cathode ray oscillograph tube will be found. With the adaptations described it has been found extremely valuable in this laboratory: (*a*) for the calibration of psychological instruments, (*b*) for the measurement of reaction times and other comparable intervals, and (*c*) for the study of physiological action potentials.

References

1. Davis, R. C., & Porter, J. M., Jr. A measuring device for the galvanic reflex. *J. Gen. Psychol.*, 1931, **5**, 115-120.
2. Gasser, H. The analysis of individual waves in the phrenic electroneurogram. *Amer. J. Physiol.*, 1928, **85**, 569-575.
3. Gasser, H., & Erlanger, J. A study of the action currents of nerves with the cathode ray oscillograph. *Amer. J. Physiol.*, 1922, **62**, 496-524.
4. Johnson, J. B. A low voltage cathode ray oscillograph. *Bell Sys. Tech. J.*, 1922, **1**, 142.

University of Virginia
University, Virginia

A MEASURING DEVICE FOR THE GALVANIC REFLEX

R. C. Davis[1] and J. M. Porter, Jr.

Since the psychogalvanic apparatus commonly used is so very faulty from the electrical viewpoint, and since the string galvanometer or other oscillo-

[1]Of the University of Virginia Institute for Research in the Social Sciences.

graph is extremely expensive the advantages of a device using the common milliammeter[2] bear investigation. The accessory apparatus here described constitutes a satisfactory circuit for the use of the milliammeter.

To secure a constant current (d.c.) through the subject and proportionality of reading to the subject's variation the present apparatus uses the principle of a stabilizing vacuum tube described by Davis (1). As shown in the diagram, however, the wiring is somewhat more simple than the earlier circuit, since the grid and plate of the subject's tube are connected together to form an anode. The subject's current is then dependent only on the filament current of the tube which is regulated by the separate rheostat (2).

In order to secure proportionality of reading to reflex the remaining circuit must be designed to draw no energy from the subject's circuit. The tube (*B*) provides a coupling of the desired sort and also amplifies the reflex. To provide still further amplification and bring the reflex with the range of a milliammeter the tubes (*c*) in parallel are added as a power stage.

The relative slowness of the rate of change of the galvanic reflex requires that a proper circuit be completely non-reactive (i.e., independent of rate of change) and forbids even the blocking condenser usual in resistance-coupled circuits. The absence of such condensers make it necessary to use some device for preventing the full plate voltage of one tube, (*B*) for instance, from reaching the grid of the next, and for keeping the voltage put into a tube at a level proper to it. This may be accomplished, as it usually is in low frequency amplifying circuits, by introducing a potentiometer into the grid circuit of each amplifying stage. The proper operating range of a CX-340 tube is from 0 to —6 volts input. The design of the circuit narrows this range somewhat, but the total voltage across the subject (*S* in the present circuit being connected to the grid of the CX-340 tube) may range from, say, 15 to 12 volts, depending on *S*'s resistance and the subject's current selected. It is not feasible, therefore, and would be fruitless if it were, to amplify that whole voltage when only a small part of it varies. So by means of the potentiometer (1) the major portion of that voltage drop is neutralized with an oppositely directed E.M.F. Likewise the potentiometer (2) keeps the input to the last stage tubes within its proper limits. Since the non-reflexing portion of the subject's resistance and E.M.F. is sure to change from time to time and subject to subject, the potentiometer (1) must be under the control of the operator. Not so the potentiometer (2) since the d.c. component of the input is constant, being simply the plate voltage of the preceding tube. This potentiometer is set at the time of original calibration and left unchanged.

---

[2]One such device, the Hathaway instrument (2), is already on the market. This uses quite a different circuit from the one here described, and appears to the writers to give readings of questionable proportionality.

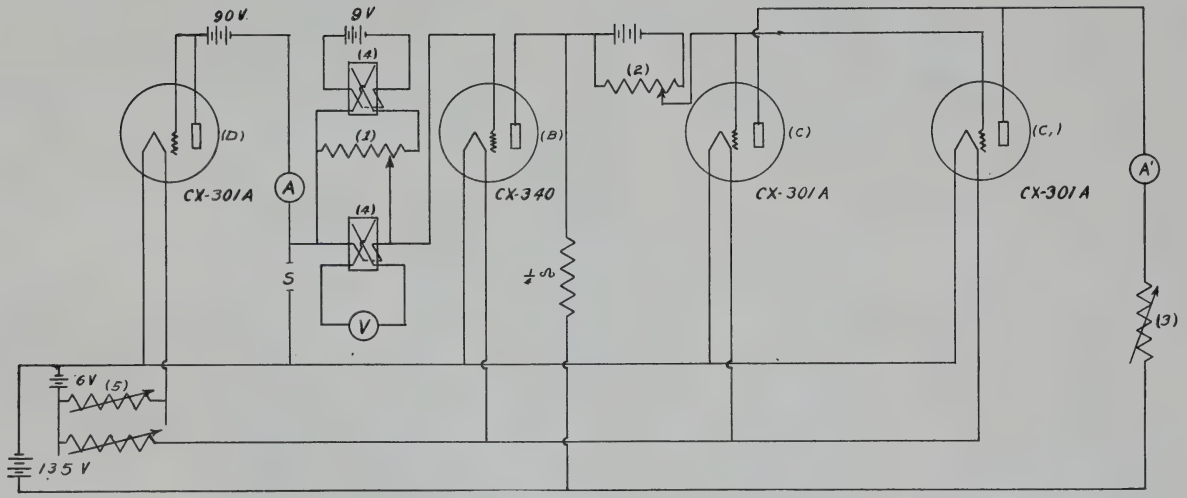

FIGURE 1

CIRCUIT USING THE MILLIAMMETER FOR INDICATING THE GALVANIC REFLEX

In order to calibrate the instrument and insure its linearity of reading, the subject's tube is turned off and a calibrated potentiometer capable of variation in hundredths of a volt is introduced on the grid of the first amplifier. A negative rather than a positive potential is now required to secure proper operation of the first tube. This is secured by means of the commutator (*A*), a two-pole double-throw switch, and about 1.1 volts used from the potentiometer thus reversed. A series of voltages is then introduced from the calibrated potentiometer and readings taken on the final meter. The potentiometers (1) and (2) and the plate resistance (3) should then be adjusted until the readings are proportional to the input voltages.

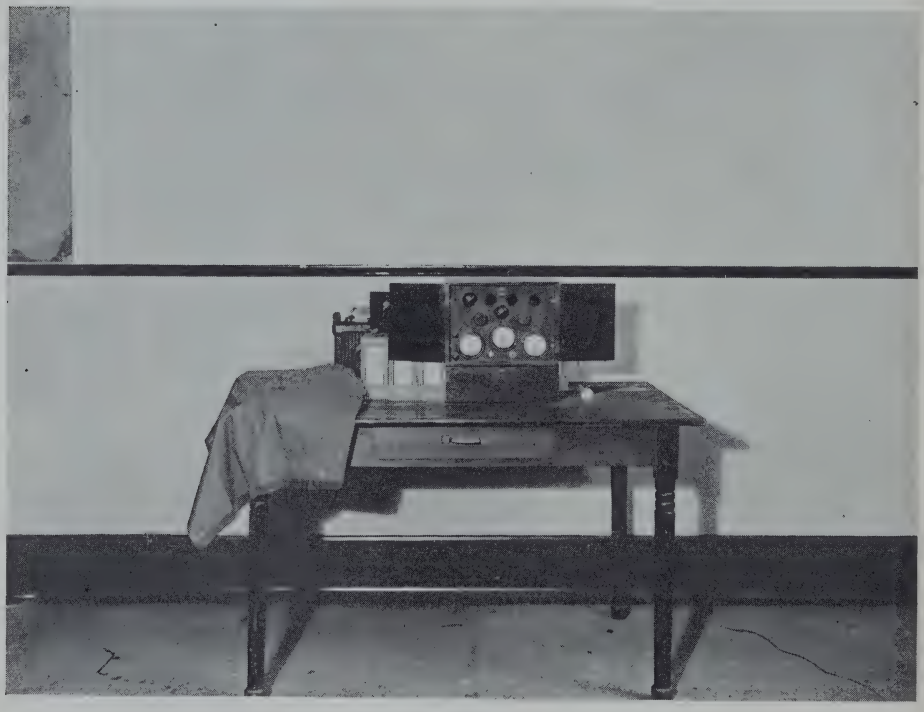

FIGURE 2

APPARATUS FOR MEASURING THE GALVANIC REFLEX

The meters, left to right, are (*A*) the subject's current meter, (*V*) the voltmeter used in computing resistance, (*A'*) the meter for indicating the reflexes. The potentiometer in the upper left-hand corner is (2) in the diagram. The rheostat below controls the subject's current. The potentiometer in the center, (1) in the diagram, controls the counter E.M.F. The rheostat on the right controls the power supply of the amplifying tubes.

This is not difficult to accomplish if one starts with about 1.2 volts on potentiometer (1), 100 volts on potentiometer (2), and 15,000 ohms in the plate resistance (3). The calibration curve may then be plotted. The voltage used from potentiometer (1) should also be measured and recorded.

It is usually desirable to know the total resistance of a subject at the moment a reflex occurs. This is obtained if one knows the subject's current, which is constant and indicated by the milliammeter (A) and the voltage drop across the subject. Now part of that voltage drop, about 1.2, is used to bias the first amplifier and the rest neutralized. Clearly the total may be obtained by adding the biasing voltage, recorded during calibration, to the amount of neutralizing voltage used. This amount may readily be measured with the voltmeter (V). (The commutator attached to the voltmeter is convenient to use during calibration.)

Thus the operating panel presents three meters, two rheostats, two potentiometers, and a variable plate resistance (if it is desired to have that on the panel). The plate resistance and one potentiometer are set at calibration and remain untouched. One rheostat is set in the ordinary manner when the set is turned on so as to supply five volts to the amplifying tubes. The other rheostat is used at the beginning of a sitting to set the subject's current at a determined value. This leaves only the potentiometer (1) to be operated by the experimenter during the sitting. With this he sets the needle of the final meter on the scale and keeps it there if it has a tendency to drift in one direction. Every time the experimenter changes the setting of this potentiometer the voltmeter (V) will, of course, give a new reading. From a record of these readings the course of the subject's total resistance can be computed. Ordinarily this potentiometer is set and a voltmeter reading taken before each stimulus is delivered to the subject.

In the apparatus described the deflection of the final meter during a galvanic reflex will be downward. This is contrary to the usual manner of representing the reflex, but logical in that the reflex is actually a decrease in resistance. The meter is prevented from damaging overload by the limiting effect of the saturation current of the tubes.

To determine what error due to its period is involved in using the milliammeter as a final instrument, a cathode ray oscillograph was connected across the final plate resistance so that simultaneous readings might be obtained from the oscillograph and milliammeter. No spreading device was used on the oscillograph spot for this purpose. Therefore the spot in response to a reflex moved across the screen and back in one line. In some observations the extent of this movement was photographed by placing a film directly on the screen, and in others the extent was simply read on a scale pasted on the screen. The calibration constant of the oscillograph was determined and the readings from it and the milliammeter were expressed in the same terms. In a series of 16 reflexes in which both meter and oscillograph were read the average error of the meter was .017 scale divi-

sions, or .0024 volts. The oscillograph deflector was photographed in 11 reflexes. The average error of the meter was .059 scale divisions. Half a scale division is about the limit of accuracy in reading.

These figures indicate that for most purposes the error in the milliammeter readings due to the period of the meter's deflection is quite negligible. The meter, of course, is inferior to the oscillograph in that it will not make graphic records to show the latent period, rising period, etc., of the galvanic reflex.

For study of the magnitude of the reflex the apparatus described possesses important advantages.

1. The deflection is proportional to the reflex.
2. The subject's current may be varied at will, but once set remains constant.
3. A continuous record of the subject's total resistance may be obtained without putting the subject into a momentary circuit for its measurement.
4. Operation is simple, involving the turning of only one knob during the procedure, and the possibility of damage through errors of operation is eliminated.
5. The apparatus is relatively inexpensive and easy to build. The present set was constructed and calibrated in several afternoons and evenings with materials costing approximately sixty dollars.

References

1. Davis, R. C. A vacuum tube for stabilizing the current during measurements of the galvanic reflex. *Amer. J. Psychol.*, 1929, **41**, 474.
2. Hathaway, S. R. A comparative study of the psychogalvanic reflex and association time measures—a new psychogalvanic apparatus. *J. Appl. Psychol.*, 1929, **13**, 632-646.

University of Virginia
University, Virginia

AN APPARATUS FOR PHOTOGRAPHICALLY RECORDING PIANO PLAYING

Joseph Tiffin

The apparatus here described was developed for the purpose of giving an objective record of piano playing. The device records photographically the time of incidence, duration, time of ending, and the relative intensity of each note played during an entire piano rendition played under completely normal conditions. In terms of these variables it is possible to describe all the significant factors in piano playing. The apparatus is at present being used in the psychology of music laboratory for the purposes of (*a*) obtaining an objective statement of the principles involved in artistic

piano performance and (*b*) recording a complex motor performance for purposes of psychological analysis.

The device is essentially a camera mounted at a distance of four feet above the bank of hammers of an upright piano. The film used is Eastman #1 Recording Paper, 4 inches wide and in rolls 190 feet long. An electric motor draws this film through the camera at the rate of 12 cm. per second. Figure 1 illustrates the action of the apparatus with respect to a single piano

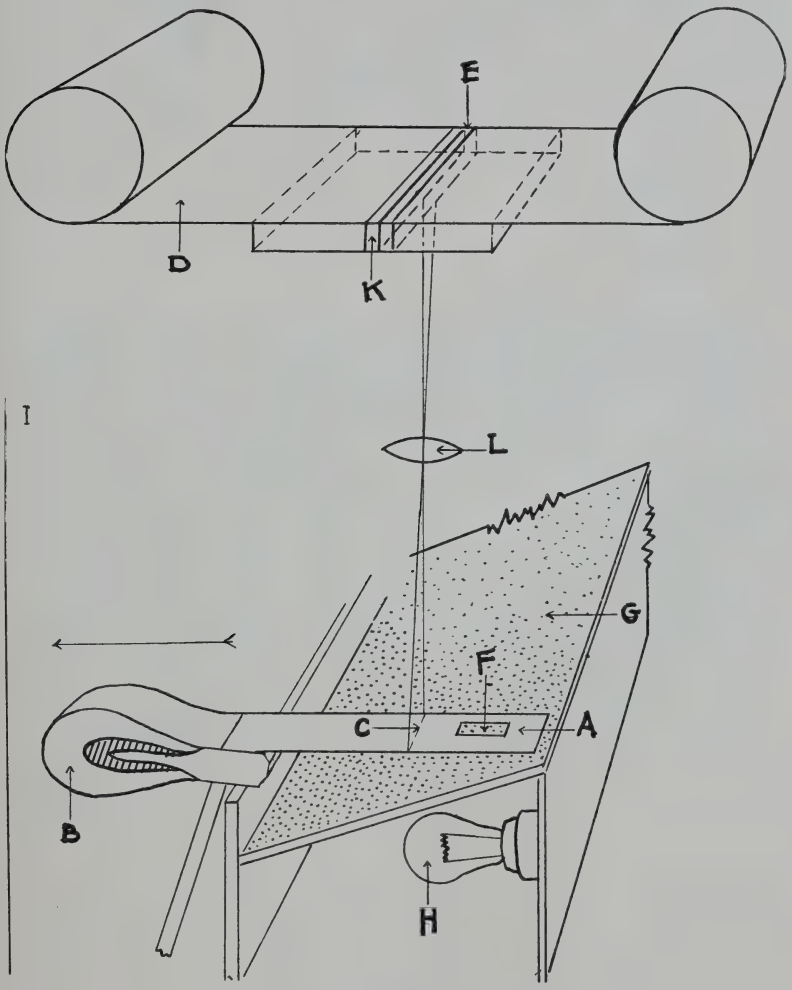

FIGURE 1

hammer. A light strip of paper approximately 7 cm. long and 7 mm. wide is glued to the tail of each hammer in the piano. The top of the piano is removed and an anastigmatic 1:6.1 lens is used to focus the region *C* of the tail *A* on the film *D*. The film is exposed to the bank of hammers below only through the narrow slit *E*. This slit is as long as the width of the film and 2 mm. in width. When none of the keys are depressed, every hammer in the piano has the region *C* of its cardboard tail focused by means of the lens *L* on the film through the slit *E*. As any given key is depressed, its hammer moves forward in the direction of the arrow and the hole *F* in the paper tail passes the region *C* which is focused on the film. Directly below the paper tails is a glazed glass (*G*) which provides a support for the tails and diffuses the light from a bank of 60-watt Mazda bulbs, only one of which (*H*) is shown in Figure 1. The bank of Mazda bulbs is placed at a distance of 30 cm. below the glazed glass. As the hole *F* in the tail *A* moves forward, the region *C* (which is focused on the film *D*) is momentarily lighted from below. The result of the passage of the hole *F* is that a dot is photographed on the moving film. The total length of the paper

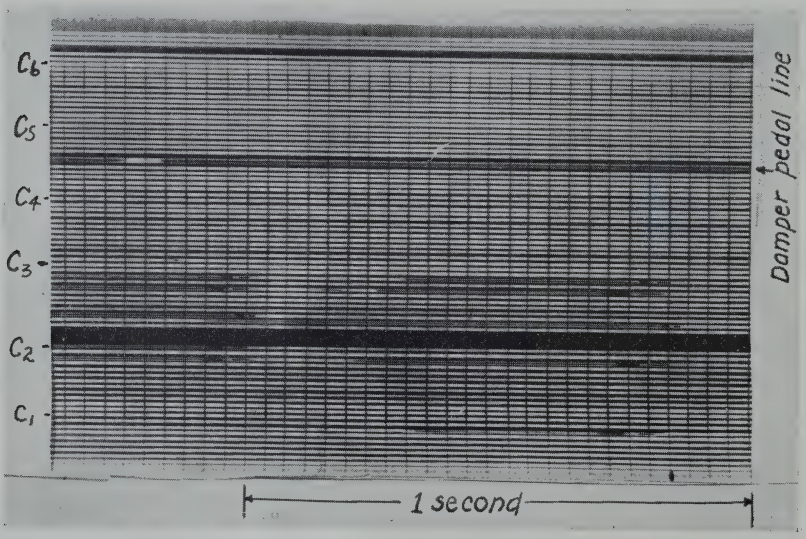

FIGURE 2

SECTION OF A RECORD TAKEN WITH THE PIANO CAMERA

Time reads from right to left. Each pathway represents a separate key. Six C's are indicated on the left as points of reference. The section shows two chords. It will be noticed that the notes of each chord are not played simultaneously or with equal intensity.

tail is such that, when the hammer strikes the piano string and produces the sound, the tail has just passed the region *C*, again allowing this region to be lighted from below and thus photographing another distinctive mark on the film. At this point in the movement of the hammer, two marks have been photographed on the film, one caused by the hole *F* passing the region *C* and one caused by the end of the tail passing this region. Since the film is being drawn through the camera during this process, it is obvious that the faster the hammer is moving, the shorter will be the distance between the two marks photographed. The hole *F* is placed at a distance of 12 mm. from the end of the paper tail. Hence the record secured of the velocity of the hammer indicates this velocity during the last 12 mm. which the hammer travels before the string is struck. It has been shown by Hickman (1) that the velocity of a piano hammer during the last 12 mm. of its movement before striking the string is practically constant. Thus, since the intensity of the tone is determined only by the velocity of the hammer, the record gives a serviceable indication of the relative intensity of the tone.

The action of a piano is so constructed that as soon as the hammers have struck their respective strings, they fall back away from the string. However, they do not fall quite back to their original position until the key is released. The position in which the hammer actually stays, as long as the key is depressed, is just enough in advance of its normal position so that the hole *F* is within the region *C*. Hence as long as the key is depressed, light passes from below through the hole *F*, photographing a narrow line on the moving film.

The procedure outlined above is exactly duplicated for every hammer in the piano, for the lens *L* serves to concentrate a picture of the entire bank of hammers on the 4-inch film. Each hammer has its own pathway on the film, and the measurement of beginning, ending, duration, and intensity of the tone contributed by each hammer may be separately made.

Although the film moves at a nearly constant rate, a time line is employed as a means of insuring accuracy in the measurements. A neon tube is exposed to the film through a slit as long as the width of the film and .5 mm. in width. This slit is adjacent to the slit *E*. The neon tube is connected to the secondary of an induction coil, and in series with the primary of the coil is placed a 25 d.v. electrically driven tuning fork and two volts of direct current. This arrangement photographs 25 parallel lines to the second, each line extending across the width of the film.

The movements of the damper pedal are also recorded on the film. As the damper pedal is depressed, a circuit is closed by means of a mercury cup. This circuit lights a flashlight bulb which is placed beside the bank of hammers and photographs a pedal line on the film in addition to the pathways of the various keys.

REFERENCE

1. HICKMAN, C. N. A spark chronograph developed for measuring intensity of percussion instruments tones. *J. Acous. Soc. Amer.*, 1929, **1**, 138-146.

State University of Iowa
Iowa City, Iowa

BOOKS

EDITED BY

KARL BÜHLER
W. J. CROZIER
JAMES DREVER
A. MICHOTTE
JOHN PAUL NAFE
A. L. SCHNIERMANN
RAYMOND H. WHEELER

FRANKLIN FEARING. *Reflex Action, A Study in the History of Physiological Psychology.* Baltimore: Williams & Wilkins Co, 1930. Pp. xiii+350. (Bibliography of 554 titles.)

It is rather unfortunate that the first book to devote itself exclusively to the history of reflex action should have been written with a particular prejudice. The reflex is perhaps the most important and the most widely used single principle of analysis in current physiological psychology. A dispassionate account of its long history could surely help to clarify future theory and experiment. But Dr. Fearing has preferred to identify himself with a particular point of view and with a particular faction in recent controversy. The special predisposition with which he approaches the subject may not be immediately apparent at first reading. On the first page, to be sure, is this quotation from Herrick: ". . . . Attention should be especially directed to the futility of attempting to derive intelligence and the higher mental faculties in general from reflexes, habits, or any other form of fixed or determinate behavior. . . . The nervous system is more than an aggregate of reflex arcs and life is more than reactions to stimuli." But the author has such a curious hesitancy in describing his own position that one must put 315 pages behind him before coming upon a definite statement of the argument. It then appears that, in this author's opinion, "although we must proceed with the greatest caution, at least we may take hope that the sterility of the faith that mind and behavior can be envisaged by number and measure has been exposed."

It would have been of great service to the reader if the author had made a statement of this position in his introduction rather than in the last paragraph of the book. Without it, many of his comments are so incoherent as to be unintelligible. For example, the chapter on the tendon reflexes closes with this paragraph:

"Whatever may be the final solution of the host of problems raised by

the numerous investigations which have been briefly surveyed in the present chapter, it is clear that the myth of the knee jerk as a simple spinal reflex is shattered. Beginning with the important investigation of Dodge in 1910 the data have been accumulating that the knee jerk, and perhaps all other "simple" spinal reflexes, *cannot be regarded as isolated units of function in the intact nervous system.*"

The "shattered myth" and the use of italics tend to leave the unsuspecting reader in confusion, especially since there has been nothing in the chapter itself except a prosaic account of a series of experiments. The presentation of the matter of the book is broken in upon periodically by these curious bursts of argument, which are, of course, intelligible in the light of the author's belated confession of his point of view, but which, as they are here encountered, are likely to be seriously disturbing to the innocent reader.

As another instance of the author's personal approach to his subject matter the introduction cites "the work of Sherrington with 'spinal' animals, the studies of Pavlov on the 'conditioned' reflexes in intact animals, as well as the investigations of Magnus, Herrick and Child," as the important work of the present century in determining the direction of theory and research. If the reader is a little surprised to find the names of Herrick and Child grouped with those of Sherrington, Pavlov, and Magnus, in this particular connection, he will be no less so in discovering that Dr. Fearing himself apparently does not regard the work of Child as worthy of a single word of description and refers to Herrick only in connection with anti-mechanist theory and other non-experimental matters.

When, upon the last page, one learns that the work is to be taken as a polemic, he must then return to the text to recover the argument. The author seems to insist upon two criticisms. The reflex is, first, an over-simplification, resulting from the relatively easy isolation of simple reflexes in the preparation, and from the fascination of the mechanical model. Secondly, it is inadequate as an explanatory principle in accounting for the behavior of the intact organism.

The first of these criticisms has been echoed by almost every writer on the reflex in the present century, and more especially by those who continue to advance it as an explanatory principle. We have Sherrington's repeated testimony to the conceptual nature of the simple reflex, and the present book quotes Watson to the effect that the reflex arc is a "convenient abstraction in both physiology and behavior." The reflex is exactly comparable to any other well-established scientific concept, from the electron to the gene. It is not observed in isolation but the notion of it is arrived at by a certain set of operations. It is the conceptual expression of a correlation between certain observed events (called, in this case, stimulus and response), and has no validity beyond this correlation. Actually, Dr. Fearing's first criticism is directed toward textbook diagrams of the reflex arc and as such is probably valid enough, although it gathers no great support from the pres-

ent discussion. Analogous criticism could be made of the contents of any first-year book in chemistry.

The author's second point is more important. The development of the notion of reflex action culminated quite naturally in its extension to include the behavior of the total organism. This, the author contends, is "naïve enthusiasm." Moreover, as we have seen, he is satisfied that any attempt to envisage mind and behavior by "number and measure" is futile. We shall not discuss this incredibly extravagant generalization; but some examination of the more limited question of the adequacy of the reflex may be given.

It is well to know what one means by "reflex action," and to this end Dr. Fearing has collected definitions from the literature and has extracted from them a table of the characteristics most often mentioned. The four most important conditions are that a reflex is (1) involuntary, (2) unlearned, (3) predictable and uniform, and (4) not conditioned by consciousness. Dr. Fearing does not indicate his acceptance of all of these characteristics. Nevertheless, he offers no definition of his own, and many references throughout the book indicate that some combination of these four elements is in his mind. Thus he accepts condition (1) when, on page 11, he comments on the Syrian Book of Medicine as follows: "The last statement would seem to be a definite anticipation of the idea of reflex action, i.e., a kind of action which is wholly involuntary." Condition (2) is implied in the discussion of the relation of the reflex to automatized action. His acceptance of conditions (3) and (4) is evident, for example, in a quotation from page 7: "Learned or habitual actions bear some of the characteristics of the traditional reflex act. They are unconscious and invariable." Moreover, he adds a fifth qualification (page 54): "In the modern usage, the term reflex is reserved for those neural arcs which do not involve the so-called higher or cortical centers."

If these five characteristics can be taken as roughly defining what Dr. Fearing means by reflex action, then it should surprise no one that reflex action, so defined, is inadequate in the explanation of the behavior of the total organism, since four out of five of the conditions of the definition exclude fields of that behavior. It is obvious that if all behavior is reflex, then it is meaningless to define reflex action as involuntary, unlearned, unconscious, or as employing limited neural tracts. The surviving condition of the definition is that of predictability and uniformity, which is, as we have seen, the statement that there is a correlation between observed stimulus and observed response which can be expressed in scientific law. Theories of reflex action from Descartes to Pavlov have never been able to point to more than some aspect of this correlation. Whether it can actually be established for the total behavior of the intact organism is certainly a moot question. The serious student will not be much concerned that Dr. Fearing has presumed upon his position as historian to judge the past and predict the future so darkly.

The *gestaltig* aversion for the reflex as an elemental concept is all the more conspicuous in that it is the only critical comment in the book. In general the author is content to piece together copious quotations from the sources—probably a full third of the book is quoted matter. Such a method is adequate enough for the early part of the history, but as the account grows more and more complex some effort at organization is called for. The author has met this difficulty with a program of rather drastic selection. He has included only those phases of the subject which are related to reflex action "as an explanatory principle in physiological psychology." What this means in practice is that (1) his discussion of inhibition barely touches the physiological literature, but turns rather to questions of attention and reaction time; (2) a chapter on the reflex maintenance of posture devotes just one page to the work of Magnus; (3) a full chapter is devoted to the relation of the reflex to motor automatisms; (4) experiments on the tendon reflexes are the only experiments discussed in detail (we have already quoted the concluding paragraph); and (5) a chapter on "Modern Concepts" gives more space to the systematic position of Koffka than to that of Sherrington, and completely ignores the position of such a physiologist as, for example, Keith Lucas or Forbes.

The early part of Dr. Fearing's book is relatively free from polemicizing. Its richness of quotation gives it a certain advantage over other treatments of the same subject. It makes available a fairly complete statement of the positions of early theorizers in their own words. Unfortunately, there is a curious garbling of the quotation from Descartes. On page 20 the translation of a quotation from the *Traité de l'Homme* is acknowledged in a footnote as borrowed from Foster's *Lectures on the History of Physiology.* The footnote continues: "See also the Tannery edition of Descartes' *Oeuvres, Traité de l'Homme,* vol. 11, p. 130 ff. The Tannery edition is the one used in the present translations. The writer is indebted to his colleague, Dr. E. L. Clark, for assistance with the English translation from the *Traité de l'Homme.*" The next quotation is also verbatim from Foster, but no acknowledgment is made. In the next two quotations from the *Traité* the author offers his own translation, although both passages are also translated by Foster. One is naturally curious, therefore, as to why the Foster translation was rejected. Reference to the Tannery edition serves only to complicate matters: where Foster has been faithful to his original in both letter and spirit, the parallel translation by Dr. Fearing exhibits no less than four misreadings of the text. The situation is repeated on pages 24 and 25 where 6 lines of Foster's translation are used without acknowledgment and 14 available lines are rejected in favor of the author's own (less felicitous and again inexact) translation.

This book appears to be neither good history nor good polemic. It seems obvious that the difficulty lies in trying to do two things at once. The historical side suffers because the polemicist must select and emphasize with

bias. And the polemic is no less obscured by the historical trappings, which, although they may be of slight importance for the argument, cannot be ignored. It is certainly of no weight in the criticism of a modern concept to show that an earlier expression of the same idea is inconsistent, and there seems to be no other reason why an out-and-out polemic needs the disguise of the historical survey. If Dr. Fearing's argument has any particular merit, it might well have been given the dignity of a separate expression. As it stands, it is unconvincing and consists in little more than the mere assertion of the sterility of the concept of the reflex.

In what respect the concept of mechanistic analysis (which Fearing uncritically tends to identify with the notion of the reflex) is sterile, is nowhere made clear. It would be distinctly interesting to follow an attempt, by Dr. Fearing, perhaps, to demonstrate just how mechanistic analysis fails to meet the conditions of scientific explanation for problems in which alternative modes of interpretation are held to be successful. Lacking the support of some such systematic investigation, objections of the type voiced in this book are little more than an appeal to ignorance.

B. F. Skinner and W. J. Crozier.

Laboratory of General Physiology
Harvard University
Cambridge, Massachusetts

Joseph Needham. *The Sceptical Biologist.* New York: Norton, 1930. Pp. 270.

The reconcilers are often with us. Some are less noxious than most. This book is in fact done less than justice by its jacket-blurb. It is announced as one for the "general reader" by a Catholic author (apparently with ecclesiastical schooling), who "reconciles the mechanistic theory with religion and philosophy." The scepticism of the title refers to the author's rejection of neo-vitalism within the circle of "conceptions proper to science," and to his acceptance of neo-mechanism as the view universally applicable in biology as in other sciences but without claim to "validity of a philosophical kind." What this sort of thing does to "philosophy" is obviously quite another story!

The book is made up from essays published in various journals and includes parts of a previous little book *Man a Machine.* The writing is distinguished by a pretty resourcefulness in recourse to the trappings of classical scholarship, and by an acquaintance with remote and uncommon books quite as unusual in the writings of biologists as it is in their ordinary conversation. Needham holds that technical "materialism" is no obstacle to "the spiritual life," but he is not one of the timid souls who write about Science searching out God. He frankly recognizes that, while apparently

the historically unkillable notion of materalism has been the doctrine of "lesser men" (as contrasted with the great ones of philosophy), it always retains vitality and is indeed "science itself." Hence, the foolishness of such phrases as "The Religion of Science." We have had a good deal of the latter rubbish of late, but there are healthy signs that no one worth worrying about is seriously deceived by it. Needham's way out is to recognize a "legitimate methodological materialism of science," and to maintain that this is no barrier to the "spiritual life" (whatever *that* may be for the given individual). And there you are! If this be "reconciliation," make the most of it. The demonstration of the compatibility of these elements is made to turn upon the well-tried proposition that science, essentially analytical, mathematical, mechanical, is none-the-less a subjectively grounded methodology and its picture of the "world" is not "the world as it really is" [what is that?] and does not exclude other views. The notion that science "is" merely analytical, with the implication that it must be so, is surely not warranted. Synthesis has been rather slighted, perhaps, but even now cannot be ignored.

These essays will interest biologists because the author himself labors in several fields of physiology and is able to give a rational exposition of the working faith of practically all sentient biologists, grounded as it is in a "primitive interest in the external world" and not bothering greatly about the "principles of scientific method." They may query the necessity of so much ado about the recognition of curiosity and wonder as compatible but not synonomous motives, and they may not regard as inescapable the identification of wonder with "the sense of holiness"; but they will approve the skillful and suggestive comment upon more concrete problems. In his presentation of "The Hope of a Chemical Psychology" Needham develops the thought that for physiological psychology "it is now bio-chemistry and not classical physiology which is important." Just how precise is this? By "classical physiology" there is apparently to be understood the physiology of nerve and muscle, gland, and spinal reflex, for the notion of General Physiology, one suspects, and notwithstanding Bayliss, has not yet been absorbed in England. Needham's biochemistry, "committed as it is to exact physio-chemical notions, interposes nothing between the affairs of living organisms and the fundamental properties of matter." It is therefore not synonomous with General Physiology, and its failure to include the problems of architectonics perhaps arises from the same source as does his desire to exclude "organism" from the list of *scientific* biological concepts. In spite of this, he says that "in this way the psycho-physical problem emerges with especial clearness out of the clouds." Does it? The neo-mechanism earlier alluded to leads him to the position that "the problem of the relation between body and mind is unanswerable, because to ask it implies that the words we use for dealing with each half represent something real." Yet he feels that the facts already known regarding human

mentality in relation to changes in body chemistry make it unwise to suppose that the mechanistic conception "can be arrested at the confines of the mind"; but he sees nothing in this for philosopher or theologian to regret, because "the philosopher and the poet have other access to truth." Do they? Certainly the assertion is no demonstration; although the philosophical dog-pit does provide kinds of truth at choice.

One hears a good deal about "organism" and "organicism" in current biological speculations. Needham gives a useful account of the history of these notions, and a good deal of attention is accorded Haldane's use of them. Biologists do not know "life"—they do see organisms, individuals. To Haldane, the "organism as a whole" is the essence of the matter, and defies "mechanical explanation." Separate it into pieces, and nothing of any value can be found out regarding it. But why the supposition that mechanistic analysis involves a physical disruption of the individual? Does it in some degree arise as a consequence of that "classical physiology" which investigated the properties of dying frog's muscles and nerves? The doctrine of "no interference" is in fact an essential tool of physiology, although its existence is not dwelt upon in this book; and its resultants are certainly as mechanistic as you please. Haldane's "emergent" organicism partakes of the slightly moth-eaten fallacy of the H_2, O_2, H_2O illustration. Needham, for other reasons, abandons it to adopt Whitehead's more inclusive conception of organism. This he regards as an instrument of philosophy, however, and not as a scientific device, which seems to be a useful means of permitting everyone to go his way in peace.

W. J. Crozier

Laboratory of General Physiology
Harvard University
Cambridge, Massachusetts

M. Rose. *La Question des Tropismes.* (*Les Problèmes Biologiques,* XIII) Paris: Presses Universitaires de France. Pp. vii+469. (90 figures.)

An analytical summing-up of the publications dealing (or purporting to deal) with the problem of tropistic conduct has been needed for some time. This monograph by Maurice Rose, Volume XIII of the series "Les Problémes Biologiques" issued under the editorial direction of Mayer and Fauré-Fremiet, does not completely supply this want; but it is distinctly useful. Its utility is not improved, however, by the absence of an index—so commonly lacking in French books of this sort—nor is the arrangement of the bibliography in sections at the ends of the chapters likely to prove especially convenient. The citations are drawn from a wide field, and the proportion of minor inaccuracies and misprints is probably no greater than usual.

The plan of the book devotes 130 pages to a review of tropistic phenom-

ena in plants, pages 130-357 to a consideration of sundry tropisms of animals; this summary of experimental findings is followed by a lengthy consideration (pp. 358-460) of the theoretical consequences of the accumulated observations. The treatment is not exhaustive, nor particularly original. It is noteworthy among European writings, however, not only for the extent of the material read, or at least abstracted, in the course of its preparation, but also for the fact that it gives some indication of realizing the logical and analytical significance of quantitative experimentation quantitatively evaluated. At the same time it must be said that the mode of presenting mathematical aspects of the subject is not especially helpful, owing to the omission of the derivations of formulae given and illustrated. It is often presumed by those impatient with even elementary mathematics that formulation represents a distortion of facts—perhaps because it can reveal unsuspected relationships!—or is used as a kind of magic. To encourage the bias of those who do not care to examine the logical basis for quantitative treatment is somewhat unfortunate. One gathers that the author's primary interest does not lie here. The illustrations of quantitative results, none of which are original, are restricted to graphical representations. In a work of this kind it should be important to provide the actual numerical data. The fact that these data may be conveniently accessible in *Tabulae Biologicae* and its Supplements (which Rose does not cite) is of no great help, because there the means of evaluating the significance of the data is not supplied through critical discussion and comparison with other relevant material.

The classical observations on tropistic response in plants are rather elaborately reviewed, chief attention being given, of course, to geotropism and to phototropism. Some of the most recent work is included. The author thinks that only the future can tell whether a nervous system in the sense of Bose may not exist in certain plants; but this opinion seems to be arrived at as a result of abstracting the literature rather than through an evaluation of the facts.

Rose's own work has been with the movements of plankton organisms, such as copepods, and to a lesser extent with observations on galvanotropism. His major interest is plainly with the qualitative facts of tropistic movements, their reversals, and mutual influences. He considers in comprehensive review phenomena of phototropism, galvanotropism, geotropism of animals, and several others in necessarily briefer style. Stereotropism he regards as a "false tropism," because to include this term among the "true tropisms" would result in a "fatal enlargement" of the meaning of tropism; but he does not tell why this is held to be so. This seems to result from a confusion of which some further consequences appear later in his discussion of theory. At any rate, the exclusion carries with it suppression of references to papers containing evidence fully justifying dissent from his position. No mention is made of the phenomena of stereotropic equilibration in various animals. Apparently "a tropism" is regarded as an element of

conduct, part of the time; at others, as an oriented movement satisfying certain definite criteria. By the latter standard—the only usable one—stereotropism certainly cannot be excluded from the category.

The final section of the book is devoted to the significance of tropisms for general biological concepts. Rose points out that the work in connection with this topic has had the advantage of definite objective and increasingly rigorous standards of experimentation, and that it leads, for example, to a more penetrating view of excitation and of the resultants of stimulation. The discussion of these points is not very strong. There is some tendency to set down the results reported by various investigators, who may be in disagreement, without realization of their incompatibility. Use is made of the notion that Le Chatelier's Theorem may be applicable to these phenomena of response, without realizing, apparently, the force of the fact that what is reduced as a consequence of excitation is not the intensity of stimulation, but its unsymmetrical character. The restoration of the "original state" of an organism asymmetrically excited, through its subsequent orientation, has nothing to do with the equation of state of a system transferred from one pressure, let us say, to another. To invoke Le Chatelier's principle here is to deal needlessly in rhetorical physics, and to play fast and loose with the sadly overmilked term "adaptation."

Rose holds that if tropistic behavior plays an important part in the adaptive relations of natural history, it is not "le fondement unique, comme Loeb l'a prétendu." Loeb did maintain that the "preservative instincts" had a tropistic character, at least in many cases; but to argue in this field requires elaborate precautions in the way of definition. In point of fact, Loeb gave pretty adequate illustrations of what he had in mind as "preservative instincts," and his view is easily supported. The comments on the relation of *tropism* to *instinct* and to *reflex,* and upon the attitude of psychologists in these matters, are neither very novel nor exhaustive, but are given in a quite reasonable way. It would be worth someone's time, if only as a source of amusement, to collate the treatments of tropism by psychologists; a difficulty is, that the account would very probably degenerate into an essay on the significance of tropisms for "Psychology," rather than for the advancement of understanding in the realm with which psychologizing deals; in any case, the devastating innocence of remarks to be encountered in well-known psychological books deserves some sort of recognition!

With regard to "error and trial" (if I may improve the customary phrase), Rose quotes approvingly the opinion of Rabaud, to the effect that this notion complicates the problem of orientation; "sans apporter aucune indication satisfaisante, elle apporte l'hypothèse d'une 'situation favorable,' hypothèse arbitraire ou interpretation subjective d'un fait relativement simple." In this connection one notes that during a protracted exposition of Loeb's conception of orientation, in which brief attention is paid to the kinetics of photic excitation and the mechanism of receptor adaptation, Rose remarks à propos of a recent contribution by Mast that, "partisan convaincu

des idées de Jennings, à l'origine de ses recherches, il arrive peu à peu, à la suites de longues années de travaux remarkable, à les conceptions purement physico-chemique qui le rapprochent incontestablement de Loeb." One only wonders at the "purement"—or perhaps the emphasis is upon intention?

The most general objection which Rose opposes to Loeb's doctrine is, that it "confond manifestement l'orientation d'une part, et l'attraction (ou la répulsion) exercée par les stimulants d'autre part. Le tropisme est identifié avec les mechanismes qu'il emploie pour se réaliser." On this basis, when phototropism is reversed, is the "attraction"? In view of statements earlier in the book, it is possible to see where this mixture of errors arises. For Rose, the "attraction" or "repulsion" is primary, and the mode of utilization of locomotor devices is entirely a subordinate matter. This follows close upon a paragraph in which Rose states that the doctrine does not lack logical support; that it is applicable to a great variety of incontestable facts; that it has appealed to eminent investigators, botanical as well as zoological. The author's difficulty with it seems to be, then, that he is worried about "the tropism" as element of conduct in a complex world, as distinct from a mode of description of a form of behavior under specified conditions. Hence the feeling that "tropism" is too simple, too schematic. I have elsewhere pointed out that the rationalization of the conduct of an organism in a complex and fluctuating environment can perhaps make progress through the study of behavior under conditions such that more than one orienting force is at work. When experiments of this sort are attempted, the illusion that tropistic analysis is "oversimplification" becomes amusing. Those who are primarily naturalists apparently want "solutions" at once, in a bound; and they are likely to assume that others have similar intent. The *problem,* not the phenomenon, is the thing which analysis simplifies—without such treatment progressive understanding is impossible. When a physiologist investigates the phototropic behavior of *Daphnia* he is not necessarily trying to explain the diurnal wanderings of these creatures in somebody's fish-pond. As a naturalist, he may very well be curious about the vertical migrations of the plankton, but when he is measuring phototropic response he is considering properties of just that and nothing more.

As net result of his compilation, Rose none-the-less regards the doctrine of tropisms as "d'une fécondité incomparable," and holds that it "a provoqué l'éclosion d'une foule de travaux, parfois d'importance capitale, de tout premier ordre dans les multiples directions. Sa fécondité est d'ailleurs fort loin d'être épuisée." Yet he thinks it safer not to exaggerate the "theoretical importance" of the results; just what this means is not made clear—"importance" for what? Perhaps it is implied merely that significant theories endure growth and modification with passage of time.

W. J. Crozier

Laboratory of General Physiology
Harvard University
Cambridge, Massachusetts

ANDREWS, E. G. The development of imagination in the preschool child. *Univ. Iowa Stud.: Stud. Char.,* 1930, **8**, No. 4 (First Series, No. 191). Pp. 64. $1.00.

ALLEN, A. H. B. Pleasure and instinct: a study in the psychology of human action. New York: Harcourt, Brace; London: Kegan Paul, 1930. Pp. x+336.

ARCHER, C. P. Transfer of training in spelling. *Univ. Iowa Stud.: Stud. Educ.,* 1930, **5**, No. 5 (First Series, No. 180). Pp. 64. $.50.

BEISWANGER, G. S. The character value of the Old Testament stories. *Univ. Iowa Stud.: Stud. Char.,* 1930, **3**, No. 3 (First Series, No. 187). Pp. 63. $1.00.

BERNE, E. V. C. An experimental investigation of social behavior patterns in young children. *Univ. Iowa Stud.: Stud. Child Welfare,* 1930, **4**, No. 3 (First Series, No. 175). Pp. 93. $1.00.

BURROWS, C. N. Criminal statistics in Iowa. *Univ. Iowa Stud.: Stud. Soc. Sci.,* 1930, **9**, No. 2 (First Series, No. 182). Pp. 112. $1.00.

CARRIGAN, R. A. Carrigan score card for rating teaching and the teacher. Yonkers, N. Y.: World Book Co., 1930. $1.00 per pkge. of 25.

CASE, R. T. A study of the placement in the curriculum of selected teachings of the Old Testament prophets. *Univ. Iowa Stud.: Stud. Char.,* 1930, **2**, No. 4 (First Series, No. 184). Pp. 54. $1.00.

COGHILL, G. E. Anatomy and the problem of behaviour. Cambridge, Eng.: Cambridge Univ. Press, 1929. Pp. xii+113. $3.00.

COGHILL, G. E. Anatomy and the problem of behavior. New York: Macmillan, 1930. Pp. xii+113.

COLEMAN, E. H. The shutter of snow. New York: Viking, 1930. Pp. 245. $2.50.

CRAWLEY, S. L. A syllabus for the first course in experimental psychology. New York: Holt, 1930. Pp. 177. $1.20.

CURTI, M. W. Child psychology. New York: Longmans, Green, 1930. Pp. x+528. $3.20.

DEWEY, J., LEWIS, C. I., LOEWENBERG, J., LOVEJOY, A. O., MCGILVARY, E. B., MONTAGUE, W. P., PARKER, DE W. H., PERRY, R. B., PRATT, J. B., ROGERS, A. K., SANTAYANA, G., WELLARS, R. W., SINGER, E. A., JR., STRONG, C. A., TUFTS, J. H., URBAN, W. M., WENLEY, R. M., & WOODBRIDGE, F. J. E. Contemporary American philosophy. Volume II. [Ed. by G. P. Adams & W. P. Montague.] New York: Macmillan, 1930. Pp. 447. $6.00.

ELLIS, W. D. Gestalt psychology and meaning. Berkeley, Calif.: Sather Gate Book Shop, 1930. Pp. xii+172.

GOW, C. R. Foundations for human engineering. New York: Macmillan, 1930. Pp. xiv+226.

FISHBACK, E. H., & KIRKPATRICK, E. A. Conduct problems for junior-high-school grades. Boston: Heath, 1930. Pp. v+60.

FLÜGEL, J. C. The psychology of clothes. (Int. Psycho-Analytical Library. Jones, E. [Ed.] No. 18.) London: Hogarth Press & Institute of Psycho-Analysis, 1930. Pp. 256.

GERBERICH, J. R. A personnel study of 10,000 Iowa high school seniors. *Univ. Iowa Stud.: Stud. Educ.,* 1930, **5** (New Series, No. 177). Pp. 112. $1.00.

GESELL, ARNOLD. The guidance of mental growth in infant and child. New York: Macmillan, 1930. Pp. xii+322. $2.25.

GILLILAND, A. R., MORGAN, J. J. B., & STEVENS, S. N. General psychology for professional students. Boston: Heath, 1930. Pp. viii+439.

HAGGERTY, M. E., OLSON, W. C., & WICKMAN, E. K. Haggerty-Olson-Wickman behavior rating schedules. Yonkers, N. Y.: World Book Co., 1930. $1.00 per pkge. of 25.

HAMER, O. S. The master farmers of America and their education. *Univ. Iowa Stud.: Stud. Educ.,* 1930, No. 2 (First Series, No. 193; Adult Educ. Series, No. 1). Pp. 152. $1.25.

HIGHTOWER, P. R. Biblical information in relation to character and conduct. *Univ. Iowa Stud.: Stud. Char.,* 1930, **3**, No. 2 (First Series, No. 186). Pp. 72. $1.00.

HILDRETH, G. H. Psychological service for school problems. (Terman, L. M. [Ed.] Measurement and Adjustment Series.) Yonkers, N. Y.: World Book Co., 1930. Pp. xvii+317. $2.16.

HOLLINGWORTH, H. L. Abnormal psychology: its concepts and theories. New York: Ronald Press, 1930. Pp. xi+590. $4.50.

KROH, O. Die Psychologie des Grundschulkindes. Friedrich Mann's Päd. Mag., 1930, No. 1122. Päd. Untersuchungen (V. Reihe: Untersuchungen zur Psychologie und Theorie der Bildung), No. 1. Pp. 352. 6.60 RM.

LALANDE, ANDRE. Les illusions evolutionnistes. Paris: Alcan, 1930. Pp. xi+464. 50 fr.

LAMB, F. W. An introduction to human experimental physiology. London: Longmans, Green, 1930. Pp. xii+335. $4.00.

LARSON, R. C. Studies on Seashore's "Measures of musical talent." *Univ. Iowa Stud.: Ser. on Aims & Prog. of Res.,* 1930, **2**, No. 6 (First Series, No. 174). Pp. 83. $.75.

LOCKHART, E. G. The attitudes of children toward law. *Univ. Iowa Stud.: Stud. Char.,* 1930, **3**, No. 1 (First Series, No. 185). Pp. 61. $1.00.

LUCAS, D. B., & BENSON, C. E. Psychology for advertisers. New York: Harper, 1930. Pp. xv+352. $3.50.

MADSEN, I. N. Educational measurement in the elementary grades. Yonkers, N. Y.: World Book Co., 1930. Pp. x+294. $2.00.

MCCONNELL, R. E. A history of the development of the Department of Public Instruction in Iowa. *Univ. Iowa. Stud.: Stud. Educ.,* 1930, **6**, No. 1 (First Series, No. 188). Pp. 122. $1.00.

MILLER, L. W. An experimental study of the Iowa placement examinations. *Univ. Iowa Stud.: Stud. Educ.,* 1930, **5** (First Series, No. 181). Pp. 116. $1.00 .

OTIS, A. S., & ORLEANS, J. S. Standard graduation examination for elementary schools. Yonkers, N. Y.: World Book Co., 1930. $1.25 per pkg.

PALMER, G. H., ADAMS, G. P., ALEXANDER, H. B., ARMSTRONG, A. C., BOODIN, J. E., BROWN, H. C., CALKINS, M. W., COHEN, M. R., CUNNINGHAM, G. W., DRAKE, D., DUCASSE, C. J., EVERETT, W. G., FITE, W., HOCKING, W. E., DE LAGUNA, T., & LEIGHTON, J. A. Contemporary American philosophy. [Ed. by G. P. Adams & W. P. Montague] Volume I. New York: Macmillan, 1930. Pp. 450. $6.00.

PATERSON, D. G., ELLIOTT, R. M., ANDERSON, L. D., TOOPS, H. A., & HEIDBREDER, E. Minnesota mechanical ability tests. Minneapolis, Minn.: Univ. Minn. Press, 1930. Pp. xxii+586. $5.00.

ROBINSON, D. S. An anthology of modern philosophy. New York: Crowell, 1931. Pp. xiv+836. $4.50.

SMITH, J. J. Social psychology: the psychology of social attraction and repulsion. Boston: Badger, 1930. Pp. 468.

STANTON, H. M., & KOERTH, W. Musical capacity measures of adults repeated after music education. *Univ. Iowa Stud.: Ser. on Aims & Prog. of Res.*, 1930, No. 31 (First Series, No. 189). Pp. 18. $.35.

STOPFORD, J. S. B. Sensation and the sensory pathway. New York: Longmans, Green, 1930. Pp. xii+148. $3.00.

STUTSMAN, RACHEL. Mental measurement of preschool children. (Terman, L. M. [Ed.] Measurement and Adjustment Series.) Yonkers, N. Y.: World Book Co., 1931. Pp. xii+368. $2.20.

TIREMAN, L. S. Value of marking hard spots in spelling. *Univ. Iowa Stud.: Stud. Educ.*, 1930, **5**, No. 4 (First Series, No. 179). Pp. 48. $.50.

WATSON, J. B. Behaviorism. (Rev. ed.) New York: Norton, 1930. Pp. xii+308.

WENTWORTH, H. A. A quantitative study of achromatic and chromatic sensitivity from center to periphery of the visual field. *Psychol. Monog.*, 1930, **40**, No. 3 (Whole No. 183). Pp. vi+192.

WHEELER, R. H. Readings in psychology. New York: Crowell, 1930. Pp. x+597. $3.75.